



DECEMBER 1961

Current Anthropology

A WORLD JOURNAL OF THE SCIENCES OF MAN

INTEGRATION OF CULTURE: A REVIEW OF CONCEPTS by <i>Elizabeth E. Hoyt</i>	407
MORE ON UPPER PALAEOLITHIC ARCHAEOLOGY	427
PHYSICAL ANTHROPOLOGY IN CZECHOSLOVAKIA: RECENT DEVELOPMENTS by <i>Milan Dokládál and Josef Brožek</i>	455
THE VOICE OF THE AMERICAN INDIAN: REPORT ON THE AMERICAN INDIAN CHICAGO CONFERENCE by <i>Nancy O. Lurie</i>	478
THE KISORO PATTERN OF MOUNTAIN GORILLA PRESERVATION by <i>Raymond A. Dart</i>	510
CALENDAR	inside back cover
CONFERENCES	508
DISCUSSION AND CRITICISM	501
INSTITUTIONS	507
JOURNAL CONTENTS	503
OFFERED	502
OUR READERS WRITE	405
PUBLICATIONS RECEIVED	505
RESEARCH REPORTS	510
SERIAL PUBLICATIONS	502
SUPPLEMENTARY LIST OF ASSOCIATES ..	512
WANTED	507
INDEX TO VOLUMES I AND II	516

Current Anthropology

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CURRENT ANTHROPOLOGY est une revue de langue anglaise paraissant six fois par an. Elle s'adresse aux spécialistes du monde entier qui se consacrent aux disciplines anthropologiques (anthropologie sociale, culturelle et biologique), à la linguistique, à l'archéologie et à la préhistoire. Les membres associés de CURRENT ANTHROPOLOGY forment un groupe de spécialistes qui aspirent à un échange scientifique à l'échelle mondiale, et qui dans un esprit d'étroite collaboration, se communiquent mutuellement des informations et des idées par l'intermédiaire de cet organe. Les institutions associées de CURRENT ANTHROPOLOGY assument la même obligation. Les deux catégories d'Associés reçoivent la revue et payent seulement le 20% de l'abonnement parce que leurs activités entraînent pour eux des dépenses supplémentaires. Les étudiants d'anthropologie recommandés par les Associés obtiendront un abonnement à prix réduit (40% du tarif normal). Qui-conque le désire peut s'abonner au tarif normal.

Historique du projet

Depuis sa fondation en 1941, la Wenner-Gren Foundation for Anthropological Research a reconnu l'importance des échanges internationaux dans le domaine des diverses sciences de l'homme. C'est ainsi qu'en 1952, la Wenner-Gren Foundation a accordé son patronage à un Symposium international d'anthropologie dont un des résultats a été la publication de *Anthro-*

pology Today, Appraisal of Anthropology Today et l'*International Directory of Anthropological Institutions*. Faisant suite à ces ouvrages, un *Yearbook of Anthropology* a été publié en 1955 à titre expérimental. Une partie de cet ouvrage a été réédité sous le titre de *Current Anthropology* qui est devenu celui de la revue.

La présentation et le contenu de la revue CURRENT ANTHROPOLOGY ont été discutés dans des conférences ou par correspondance avec les spécialistes du monde entier. Au cours de la conférence de Burg Warstein (Autriche), centre européen de la Wenner-Gren Foundation, les principes généraux suivants furent formulés:

1. La revue traitera de tous les problèmes intéressant les diverses disciplines anthropologiques et réunira des faits significatifs et des idées nouvelles afin de faciliter les échanges internationaux.

2. Elle visera à la synthèse.

3. Elle constituera pour les spécialistes un instrument de travail apte à leur fournir des informations sur les recherches en cours.

L'année suivante, grâce au concours de nos collègues, les principes prirent la forme du plan présenté ici.

Le Statut d'Associés

CURRENT ANTHROPOLOGY groupe à la fois des spécialistes et des institutions sans tenir compte des nationalités et des opinions politiques. En échange du tarif spécial qui leur est consenti on attend des Associés qu'ils présentent de nouveaux membres, qu'ils soumettent des articles et communiquent des informations susceptibles d'intéresser les autres Associés. La rédaction compte également sur ceux-ci pour obtenir des commentaires sur des articles de fond.

Contenu

CURRENT ANTHROPOLOGY publie deux sortes de textes: d'une part des articles sur l'état actuel d'une question, de l'autre, des informations diverses et une documentation de base. La revue publiera forcément des articles qui auraient pu trouver place ailleurs. Néanmoins le cadre géographique très ample assigné aux articles et leur orientation théorique lui éviteront de faire double emploi avec d'autres revues. CURRENT ANTHROPOLOGY en tant qu'instrument d'échanges entre spécialistes répond à une formule originale.

Articles sur l'état actuel d'une question

Ces articles porteront sur des problèmes généraux et en donneront une vue d'ensemble. Il ne s'agira pas, à proprement parler, de synthèses de faits dûment établis, mais de présentations de données inédites, d'idées ou de théories en des domaines où une anthropologie en plein développement témoigne d'une orientation nouvelle. Cependant pour neufs que seront les travaux présentés, on ne fera pas abstraction de ce qui est déjà connu. Les contributions à la revue consisteront tantôt en des réinterprétations des résultats de recherches antérieures tantôt en des examens critiques de l'ensemble de nos connaissances sur un point donné. Les limites que les auteurs s'assigneront seront fonction d'une période, d'une région géographique ou de tout autre critère. On demandera aux auteurs de formuler clairement les bases de leur argumentation ou de faire brièvement l'historique du thème qu'ils aborderont.

On ne craindra pas les grands sujets qui seront traités largement, sans pour autant négliger les explications de détail, les exemples concrets, et enfin les possibilités que réserve l'avenir. Chaque article comportera une abondante bibliographie. L'auteur qui, par définition, sera un spécialiste de la question, s'adressant à des collègues, s'efforcera d'être clair, surtout dans l'emploi de sa terminologie. L'article sera aussi long que le sujet l'exige, mais dans des limites raisonnables. Il y aura intérêt à ce qu'ils soient illustrés.

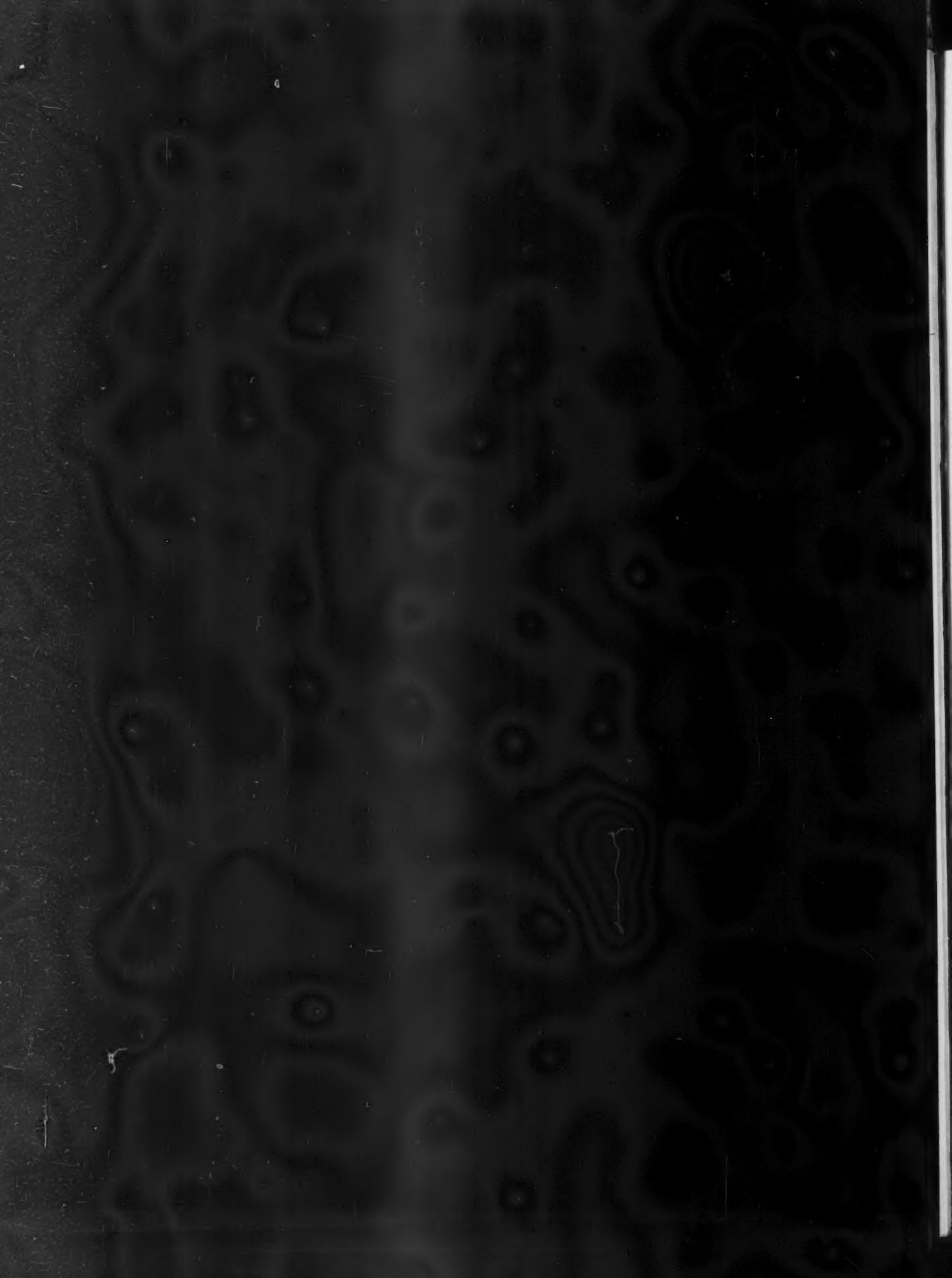
Les articles envoyés à CURRENT ANTHROPOLOGY, une fois acceptés, seront copiés en plusieurs exemplaires et expédiés aux membres associés, spécialistes en la matière ou s'intéressant de façon particulière au sujet traité.

Les commentateurs sont choisis sur la base des fiches, classées au bureau de la rédaction, et qui ayant été remplies par les Membres Associés eux-mêmes contiennent des indications sur les sujets de leur spécialité. Les spécialistes désignés peuvent aussi être recommandés par l'auteur de l'article.

Bien que ces commentaires soient sollicités, ils seront considérés comme des "manuscrits" originaux et leur publication sera soumise à la décision du rédacteur.

Un article sur l'état actuel d'une question devrait normalement compor-

Continué au verso intérieur



Our Readers Write

Since the anonymous English Associate's criticism of CA was published in Letter to Associates No. 6, I have followed the replies with interest but fail to find in them the points that seem most obvious. Boev is right in saying "it is obvious that CA is very useful and that it should exist" (June 1961: 299); this point need not be discussed here. Yet, his letter and those of Osborne and Silverberg (April 1961: 61) have brought out points that might not have been presented had not the issue been raised in CA initially. Unless anthropologists clarify their own thinking, we shall continue to be criticized by non-anthropologists for wasting time and money and for prying into other cultures' affairs. Bringing these letters to our attention furthers anthropology by crystallizing and clarifying our thinking on the matter. Moreover, by considering and analyzing constructive criticism, and by continuing to contribute to CA, we shall prevent its becoming the "periodical" of which Osborne writes, and we shall prove ourselves to be "a 'community' of scholars, rather than simply the readers of a journal" (Letter to Associates No. 7).

Both Osborne and our English colleague seem to show the same failure to grasp the basic realities of the problem of securing "original research while there is still time" on disappearing cultures (and, I may add with Silverberg, on cultures that are as yet unstudied, whether or not they are considered to be disappearing). Very few anthropologists would argue against the need for or the value of such field researches when their aim is, as Silverberg puts it, to "advance knowledge of cultural forms and processes and of human behavior"—especially if this knowledge is to be applied in an attempt to solve some of the world's intercultural problems. The question is: How are we to accomplish the goal in the shortest time and with the wisest expenditure of funds and efforts?

Our English colleague mentions "support of original research." To those of us engaged in long-term field observation and research, "support" is more often needed in the form of help and encouragement of a non-financial nature. CA, in making available the experience and research facilities of our colleagues in academic institutions throughout the world, opens up the possibility of a truly co-operative venture—a teaming of abilities and resources to attack problems of interest to all of us.

But what of Osborne's suggestion about the "training of professionals"? Among the basic realities are these: There are not enough professionals presently available for sending out; moreover, there are no prospects of there ever being enough of them, soon enough to make studies before the disappearing cultures vanish. Although Bennett (April 1961: 70) is writing about another point, his statement applies here: "the great majority of anthropologists . . . are engaged in teaching as members of academic faculties."

Within the time limits imposed by vacation periods, it may be possible for academically-employed anthropologists to reach the habitat of a people whose culture is vanishing, but it is certainly *not* possible for them to make an adequate study. Archaeologists may be able to produce creditable results in the short-term, "dry season" dig. But the study of a living culture cannot thus be felicitously pursued. Stelae may yield to the importunate pick, shovel, and lever. The accurate study of a living culture requires tools and methods other than those of brawn, importunity, and haste. The piecemeal method, if it works at all, works only indifferently here.

First, rapport must be established. This alone can well require more time than that afforded by the non-teaching months of the year. Equally true is the fact that, after each absence of the investigator, rapport must be re-estab-

lished before effective research can be begun anew. I have observed that the very urgency of the task and the shortness of available time often make the investigator impatient with a reticent people. And impatience is not the germinating ground of rapport. Moreover, rapport is a two-way road. The professional teacher may find it hard to divest his mind of its cap and gown; professorial superiority and an academic tone of voice are as out of place in a rain-forest culture as a dress tie and city shoes.

One more basic reality militates against the accomplishment of the desired "quick but complete field study": the fact of today's specialization. Shall the study include the archaeology of the area, and the ethnobotany and ethnozoology, the religion and magic, the material culture and economics—as well as linguistics and folklore? How large a team of specialists shall we send to each vanishing culture? We must realize that while deserts, mountains, or polar areas may best be studied or conquered by large expeditions with tons of equipment, this is not so of cultural studies. Here, the smaller the team or group, the better. Anthropology needs some "general practitioners." In some instances, a single investigator or husband-wife team would be the best solution. In others, a well-chosen but small team of versatile individuals could more quickly cover the ground *if* the increase in numbers would not defeat the purpose by increasing reticence in the people to be studied.

The best and most complete study would be that produced by "in residence" study over a long period of time. By this I mean that total observation of the total culture (the culture as a unit, a whole) is best done by investigators living among the people for long-term research. This research is best done through the language of the culture under study, and not through interpreters.

CA offers help to many of us who may not qualify as professional anthropologists in branches other than linguistics. (May I suggest that universities consider the value of securing more professors than they need for the classroom work, and assigning them on an alternating basis to field work? Certainly there is a crying need for more field work.) And we in the field need to keep abreast of the latest advances in related fields. Our contributions will be of increasing scope and value because of the aid we receive through the pages of CA. Could we measure progress thus secured, I am positive that it would prove to be more for a given period than could be secured either by training or paying fares to bring professionals, rather than CA, to the site for study.

There must be many persons who know much about the "unstudied" cultures, who for one reason or another are living in the area but engaged in other business. Perhaps they are "amateur" anthropology enthusiasts. With encouragement, they might be willing to write up what they know, or begin to observe, thus saving sending someone. Perhaps the CA editor that Boev suggests for each country could suggest persons for whom CA could provide the necessary encouragement. Let us find people already in the areas to be studied, and help them do the job.

WALTER S. MILLER
Mitla, Oax., México

Sharma's suggestion [April 1961: 70] of a report by Mourant, Weiner, and Boyd on the anthropological significance of population serology is very worthwhile, and I hope to read such an article in a future issue of CA. I would also suggest publishing a similar article on the significance of dermatoglyphics or fingerprints for racial anthropology, and if necessary, am willing to contribute to it.

A. G. DE WILDE
Leiden, Netherlands

With the exception of persons at a few major universities, full and complete information about C-14 dates is not available to everyone. CURRENT ANTHROPOLOGY would probably be an excellent means to make such data available to a broader audience. This should be done in two ways:

1. Short review-summaries of C-14 dates for specific subjects chosen according to culture areas and archaeological periods.

2. A section called "New C-14 Dates," to which all Associates should be asked to contribute.

EMMANUEL ANATI
Paris

I regret that most Associates object to the reprinting of articles that were published in English, French, German, or Spanish, since many scholars are unable to read all these languages; also, many interesting articles, published in Aus-

tralia, New Zealand, and elsewhere, are not generally available.

What about the idea that CA organize an office (or make arrangements with another institution) which could provide Associates with photostatic or microfilmed copies of wanted texts?

MILENKO S. FILIPOVIĆ
Sarajevo, Yugoslavia

I read with interest Edwin Ardener's letter, voicing criticism of the style and manner of presentation of CA. Curiously, I find myself agreeing with the criticisms point by point ("Bharat" was a puzzler, and if Burma were listed, "Pyidaungsu Myanma" would enlighten few), yet concluding that taken all together they amount to a triviality. I much prefer a journal that carries the stamp of definite personality to one that strives for muted anonymity. I am in favor of anything that leads to lively communication, [even] at the risk of occasional excursions into the ridiculous.

Perhaps the real reason for most of the criticisms is that CA is written, not in English, but in American. The obvious similarities between these two languages often blinds us to the fact that they are different, and straightforward prose in one resembles jargon in the other. I will not go so far as to suggest parallel publication in English and American, however.

E. PENDLETON BANKS
Winston-Salem, N.C., U.S.A.

The reading of our Journal would be a much more enjoyable occupation if contributions were all written in plain English and in a simple, understandable style. Yet, alas, this is not always the case! Let me quote a few examples:

Until very recently, the problems in North American archaeology that needed immediate attention were the definition of cultural units through classification of artifacts, and the chronological ordering of the units and artifact classes.

I read this statement several times without grasping its meaning. Finally I began to analyze: The problems . . . that needed immediate attention were the definition . . . It did not sound right. What about the following translation: "Until very recently, prehistoric research in North America has mainly

been concerned with establishing a chronology and typology of artifacts in order to determine certain cultural units. This, no doubt, was the most pressing task." Perhaps someone else can do better. Another example: the references cited by commentators being *inter-alphabetized* with the author's bibliography but distinguished by the initials of the *appropriate* commentator.

I ignore whether the monstrous verb "inter-alphabetize" does actually exist. If it does it ought to be ostracized. As to the word "appropriate" I strongly suspect that it usurps here the place of "respective."

I vaguely feel that "this conditioning is most unfortunate, because of our *built-in destruction* of our own materials," but I oppose the view that "we shall always need the *surface or strata-cut sheyd-collecting approach*." (Similar word-sea-serpents may be found in Kant's *Kritik der Reinen Vernunft* or in Spengler's *Untergang des Abendlandes*, but, at least, these German sea-serpents have a pedigree. Will monstrosities such as "built-in destruction," "feed-back reaction," or "pot-defined cultures" ever be admitted in our family of words? Everything is, of course, possible in our Ice-cream civilization, but let us at least try to fight the dragons.)

You may perhaps ask: "How can one generalize from—or even *test against*—the interpretations . . ." One may perhaps generalize "from" (though I am by no means sure); one may also *testify against*; but one cannot "*test against*." One can be "against" something, and I feel inclined to be against "These standard techniques (which) must be used to obtain *cultural time and space dimensions*," for the simple reason that I cannot be bothered to find out what these words mean. (Even the respectable term "space-time" gives me the creeps.)

All this may be said notwithstanding the fact that "these *points* have been particularly well *handled* in theory and practice by such scholars as . . ." (I envy the particular scholars for being able to "handle" points in theory and practice. All I ever succeeded in doing was to "stress" points—and this in theory only.)

PRINCE JOHN LOEWENSTEIN
Halifax

Integration of Culture:

A Review of Concepts

by Elizabeth E. Hoyt

INTRODUCTION

"INTEGRATION of culture" is a term with many meanings, all of which imply some congruity of the parts of a culture, and therefore some economy in their use. In a world where all resources—time, energy, and physical resources—are limited, the goal is that ultimate energies should be conserved and that institutions and values should, in the last analysis, be consistent with common ends. While integration of culture is, in this sense, a term pertaining to the science of economics, the concept has long—though loosely—been a concern of philosophy. The most specific contributions to this term, however, have come from the behavioral sciences. For description and analysis, the term "integration of culture" depends upon anthropology and sociology, and to some extent, history; and it depends upon psychology for interpretation of the nature and process of the interrelation of

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HOYT's interest in culture integration has been fostered by life in the Orient (1928); in Mexico, Central America, and the West Indies (1940, 1948, 1957-58); and in British East Africa (1950-51). For the past two years she has been studying economic-cultural adjustment of young American Indians.

The present article, or pertinent sections, were sent by the author for preliminary criticism to a dozen scholars, including all the living persons whose work is referred to, except in a few cases where there could be no possible disagreement as to the meaning of the reference. Submitted to *CURRENT ANTHROPOLOGY* on May 29, 1960, it was sent for CA☆ treatment to 50 scholars, of whom the following responded with written comments: David F. Aberle, Harumi Befu, Audrey Butt, Morton H. Fried, Stephen Fuchs, Geoffrey E. Gorer, Eiichiro Ishida, Felix M. Keesing, Eleanor Leacock, A. H. J. Prins, M. S. A. Rao, Surajit Sinha, Kenzō Tsukishima, Shunpei Ueyama, and Teigo Yoshida. The comments are printed in full after the author's text, and are followed by a reply from the author. References cited by a commentator but not by the author are inter-alphabetized with the author's bibliography but distinguished by the initials of the commentator.

parts. But these disciplines by themselves are not enough. Today we need an interdisciplinary science, a science of symbiotics. No culture is an island.

Approaches to the discussion of integration of culture are inductive and deductive, philosophical and mechanical, theoretical and pragmatic. The particular approaches used by various disciplines have been largely determined by their different purposes and interests, although there are certain differences within each discipline. Anthropologists and sociologists are fairly well aware of each other's work, but beyond this there is relatively little communication with other disciplines and not always much respect.

In their analysis of concepts and definitions of culture, Kroeber and Kluckhohn (1952: 159) say:

As of 1951, there appears to be general agreement that every culture possesses a considerable degree of integration of both its content and its forms . . . ; but that the integration is never perfect or complete, Malinowski and the functionalists having overstated the case, as well as Spengler and Benedict with their selected examples. . . . In any given situation, the proper question is not, Is integration perfect? but, What integration is there?

Before we ask this question, however, we had better ask what integration means.

THE MEANINGS OF INTEGRATION OF CULTURE

The term "integration" has been used by sociologists perhaps more than by any other group of scholars; but sociologists have generally been interested in the integration of society more than in that of culture, and sometimes they use the term "cultural integration" for the integration of separate parts of culture rather than of the whole. Their best-known references to integration are incidental to other purposes and some of the most notable are not clear-cut.

Integration is often used interchangeably with several other terms, of which the most common is "pattern." Pattern, as distinct from integration, is a term that emphasizes structure; this structure is perceived as a whole, whose parts are to some extent related. "Inte-

gration," while sometimes used synonymously with "pattern," has the further implied meaning of process behind structure. Whereas the term "pattern" is essentially a description, the term "integration" usually involves an interest in explanation as well, an interpretation of how interrelationship is achieved. A pattern is, at any given moment, static, though a dynamic process may lie behind it.

The most distinctive definition of integration is found among the Gestalt psychologists, most of whom agree with the basic statement of Max Wertheimer, founder of the Gestalt school, that a meaningful whole is not only different from the sum of the parts, but that the relationship among the parts is one of demonstrable mutual dependency (1938: 1-11).

This meaning of integration is distinctly different from Wissler's (1923: 13-79) "universal cultural pattern," with which it may be contrasted. All cultures, Wissler says, are made up of certain definite parts which together constitute the whole; and he lists the names of these for any culture: material traits, aesthetic interests, and so on. Undoubtedly a list of parts can be made, and a list may be very useful; but drawing up such a list does not help us to understand what integration is. Studying Wissler's list, or any list, we see that there is a degree of integration, for many traits belong in two or more categories; but integration itself is unexplained.

Nevertheless, Wissler was interested in integration. In another part of his work (pp. 50-51; 63-71) he makes a contribution to one kind of integration, that of association, which at the time of his training in psychology was the dominant explanation of the learning process. No one has gone so far as Wissler in showing how one interest leads to another through association. He presents the concept of the culture trait which expands into a trait-complex through the process of association (as the trait of maize, for instance, became a complex of scores of maize-relationships among Plains Indians); further, trait-complexes tend to adhere throughout all areas of life. Wissler's idea is like that of several cat's-cradles: In each intricate system the strings carry the associations that form the trait-complex; a few strings unite all trait-complexes themselves.

Wissler never claimed that culture integration in any large sense was achieved by such a means; and it would be impossible to explain all existing integration by association psychology. Gestalt psychology, accompanied by the psychology of the unconscious, was gaining ground toward the end of Wissler's life, but he died before he could incorporate these concepts with his previous work. Today, although we recognize that a certain kind of integration occurs through association by way of trait and culture complexes, integration is most often discussed in terms of the function performed, rather than of associations that have been or can be made.

Within this concept of function, however, are two different emphases on integration, each with variations. At one pole is the concept of integration by a single idea or value, or a group of similar ideas or values, dominating the culture. At the other pole is the concept of integration as interaction and inter-coordination among many similar and dissimilar traits, factors, interests, or values within a culture, each reacting on others

and on the whole; and not only is the whole different from the sum of its parts, but the parts derive significance from one another. This is distinctly a Gestalt concept in pure form.

Although these two concepts are at different poles, they need not be antithetical. The first concept emphasizes domination as the integrating force, and the second emphasizes interaction; but both forces can—and perhaps must—exist together.

The name "normative" has been suggested for the kind of integration that is due to the dominance of one value or a group of related values, and the name "functional" for the kind in which the parts work together as "mutually supplementary" (Freedman 1956: 171). Sorokin (1957: 54) uses the term "logically-integrated" for the first kind of integration, and "causal-functional" for the other (although he does not regard causal-functional integration as characteristic of a whole culture). Neither set of terms seems entirely satisfactory when applied to culture, because both kinds of integration can be conceived as logical and functional. It seems best to keep the distinction between integration through the dominance of central values over the parts of culture, and integration of the parts as such.

Before discussing these two approaches to integration of culture, we should note Karl Mannheim's (1952: 33-83) perceptive statement regarding the first, which relates it in some degree to the second. For Mannheim *Weltanschauung* is not to be found in any major sense in ideas which are formally put forth in a culture, but rather in its "spontaneous, unintentional, basic impulse," which is expressed in the ways its parts operate. Reality transcends thought.

The difficult and paradoxical nature of the concept of *Weltanschauung* stems from the fact that the entity it denotes lies outside the province of theory. . . .

It needed the anti-rationalist movement within the cultural studies themselves . . . to make people realize that theoretical philosophy is neither the creator nor the principal vehicle of the *Weltanschauung* of an epoch; in reality, it is merely only one of the channels through which a global factor—to be conceived as transcending the various cultural fields, its emanations—manifests itself. More than that—if this totality we call *Weltanschauung* is understood in this sense to be something a-theoretical, and at the same time to be the foundation of all cultural objectifications, such as religion, *mores*, art, philosophy, and if, further, we admit that these objectifications can be ordered in a hierarchy according to their respective distance from this irrational, then the theoretical will appear to be precisely one of the most remote manifestations of this fundamental entity. As long as *Weltanschauung* is considered as something theoretical, entire vast provinces of cultural life will be inaccessible to historical synthesis. . . . (pp. 37-38).

If, on the other hand, we recognize *Weltanschauung* to be a-theoretical, with stated values as only one of its manifestations,

we can widen our field of cultural studies in a twofold way. For one thing, our search for a synthesis will then be in a position to encompass every single cultural field. The plastic arts, music, costumes, *mores* and customs, rituals, the tempo of living, expressive gestures and demeanour—all these no less than theoretical communications will become a decipherable language, adumbrating the underlying unitary whole of *Weltanschauung*. Secondly, in addition to widen-

ing the field of studies in cultural synthesis, this approach will enable us to look at our object from an entirely new side. For we then shall be in a position to compare, not only discursive utterances, but also non-discursive elements of form; and once we do that, we shall be bound to feel that we have come far closer to the spontaneous, unintentional, basic impulse of culture than when we were trying to distill *Weltanschauung* merely from theoretical utterances in which the original impulse appears, so to speak, in refracted form. (pp. 38-39).

Mannheim's position is not new, and other scholars implicitly share the same underlying idea; but he makes its significance unmistakable.

INTEGRATION BY DOMINANCE

Ruth Benedict's *Patterns of Culture* (1934), the first work by an American anthropologist that gave much attention to the idea of integration, treats it in the sense of domination. This concept of integration, however, is very old, going back to 18th- and 19th-century German and Russian philosophers or philosopher-historians, and beyond them to certain Greek philosophers (Hughes 1952: Ch. III, IV). Suggestions of integration by dominance, arising from a different interest, also appear in Durkheim's work. Benedict herself speaks of Wilhelm Dilthey's *Weltanschauung* (1921) and Spengler's *Decline of the West* (1918). Spengler described eight main cultures, each dominated by an idea that differentiated it from the others. Three of these ideas were of outstanding importance in his work: the Apollonian of the classical world; the Magian of the Byzantine empire, with certain features of the Arabian, Islamic, Hebraic, and early Christian philosophies; and the Faustian of the modern West. A similar kind of division was made by Nicolai Danilevsky, who most directly anticipated Spengler; his work was published in serial form in Russia in 1869, and later in Germany.

There has also been considerable semipopular presentation of the idea of culture dominated by a single idea or group of related ideas. A large number of books attempt to characterize United States culture in terms of one or several related values. Among several non-American examples, we may note *What is Civilization?* by Maurice Maeterlinck *et al.* (1926). This volume includes two noteworthy essays: one by Maeterlinck himself on the culture of Egypt, where the ruling idea concerned death, burial, and after-life; and D. G. Mukerji's outstanding essay on the dominance of the linkage of subjective and receptive principles, as opposed to the objective and active, in the culture of India, the "repose which can achieve more than any movement."

Benedict's work indirectly shows how difficult it is to use the concept of dominating idea. Instead of Spengler's differentiation of cultures into several groups, she prefers to use Nietzsche's contrast of two values—the Apollonian (Apollonian) and the Dionysian. The former is marked by a middle-of-the-road ideal, and the latter by desire to achieve excess; her use of the terms, however, has connotations somewhat different from Nietzsche's. Benedict declares the Apollonian to be the ideal of the Southwest Pueblos and the Zuni in particular; the Dionysian is the ideal of other North American Indians, of whom she uses those of the Northwest Coast, particularly the Kwakiutl, as examples.

Macbeath (1952: 88-92) and others have pointed out that in making these distinctions Benedict has had to force the picture of Zuni and Kwakiutl; and that she has chosen not simply two values, but two basic human attitudes toward all values whatsoever. The weakness of her approach is shown by the failure of this limited differentiation between moderation and excess to help us understand Zuni and Kwakiutl in comparison with other cultures—whether Indian, other so-called native cultures, or the white American culture which is impinging upon both.

For her third culture Benedict chooses the Dobuans, a small group of Melanesians. Although here she drops her Apollonian-Dionysian distinction, we must assume from her description that the Dobuans are Dionysian. She characterizes them as a people whose culture is dominated by suspicion, cruelty, and treachery. In fact, her description (p. 172) makes them probably the most disagreeable people in the literature of anthropology:

The Dobuan lives out without repression man's worst nightmares of the ill-will of the universe and according to his view of life virtue consists in selecting a victim upon whom he can vent the malignancy he attributes alike to human society and to the powers of nature. . . . Suspicion and cruelty are his trusted weapons in the strife and he gives no mercy, as he asks none.

In *An Anthropologist at Work* (1959), Mead tells how Benedict looked for a third illustration of her thesis and finally took the Dobuans on the basis of Fortune's (1932) *Sorcerers of Dobu* and conversations with him. This choice must indicate the difficulty she had in finding what she regarded as clear cases of integration; Malinowski, in his introduction to *Sorcerers of Dobu* (p. xxviii) characterizes the Dobuans as a "people more attractive than any Melanesians I ever came across."

Although Benedict was the first American anthropologist to draw attention to the idea of integration in culture, and the use of the term "pattern" in relation to culture owes much to her efforts, she makes no wide claim for the theory. In fact, she says (p. 223) that "lack of integration seems to be as characteristic of certain cultures as extreme integration is of others."

Before the publication of *Patterns of Culture*, at least one economist had contributed to the concept of culture as being dominated by a single value or group of related values. In *The Consumption of Wealth*, Elizabeth Hoyt, after declaring that an economist can understand consumption only through understanding the culture in which it takes place, gives as part of the definition of culture "beliefs, practices, goods and services linked and bound together . . . by a mutual dependence on some common point of view" and adds (1928: 9-10):

All goods and services consumed except the very simplest—air, light, and the food and protection necessary to keep a man alive—and sometimes even that—enter human experience not as things in themselves but as things refracted by a point of view, as light is refracted by passing through a prism. As yet each main type of culture is its own dome of many-colored glass.

The approaches used by the Russians, Germans, and Benedict herself are primarily, though not exclusively, inductive; but deductive approaches have also been used. Thus Hoyt sets up four different main world-views on the basis of an active or aggressive way of meeting life versus a passive or receptive one: the first way generally characterizing the West, and the second the Orient—or the Orient as it used to be. In addition to this distinction, some cultures are concerned primarily with externals, objects of the material world; and others with thought, feeling, and insight. The culture of the United States is clearly characterized by activity in the way it meets life, and its interests are largely expressed in material or technological form. The people of classical Athens also met life actively, but their activity involved intellect and insight, and was expressed in the idea. Mukerji's classification of India's culture as characterized by repose "which can achieve more than any movement" was referred to above; it was passive or receptive in the way it met life, and its interests were primarily subjective. Chinese culture also was passive or receptive rather than aggressive, but in contrast to India it put much emphasis on things that could be seen and handled, and on organization and arrangement: aestheticism extended to the most minute detail of the humblest material objects. Whereas India's attitude was that the spirit took care of the form, China leaned to the assumption that the proper form assured the proper spirit (Wilhelm 1928).

This kind of differentiation is serviceable, but its service is limited. Obviously there is a major difficulty in describing whole cultures in terms of one dominant value or group of related values, because there may not be agreement as to the characterization of a culture by a basic value, and also because we cannot find basic-value terms to distinguish more than a few cultures or types of cultures from one another. This approach serves very well to differentiate China from India (as they used to be), and both from the modern Occident; but this kind of dominating value is not very useful for differentiating parts within these great areas.

Sorokin (1957: 24–52) uses a deductive approach with many subheads, which he proposes as a "fruitful way of ordering the infinite chaos of cultural phenomena, at least in their inner aspect, into a few comprehensible systems." Using what he regards as major premises, he would make the main division between ideational and sensate types of culture, with subheads under each, and mixed types composed of elements of both ideational and sensate. When he discusses their applications, however, he is clearer when he applies them to the cultural mentalities of individuals and small groups rather than to cultures as a whole. In a later work, *Power and Morality*, in which he is primarily interested in welfare and survival, Sorokin (1959: 184) considers only the ideational type constructively integrated, and uses the term *integral* as contrasted with *sensate*.

INTEGRATION BY PARTS AS SUCH

The impossibility of characterizing every culture by a dominant value that will set it off from every other culture has naturally led to a search for sets of values that will characterize a culture when they are taken as a whole. This is the purpose of the Harvard Values

Project, the theory of which is set forth by Clyde Kluckhohn (1956). There are certain basic clusters of relationships in all cultures, including relationships of man and nature, of man and man, and of now and then. First we decide what these clusters are. Then, for each culture, we find the different kinds of emphases for the different values which comprise the clusters. Kluckhohn (1956: 119) speaks of "the more general or thematic value-tones or value-emphases that constitute the structure points of whole systems of cultural values," of "key values that give different cultures each a distinctive quality," and of "cross-cultural core value emphases."

Thus we set up a framework of terms and of kinds of responses which is universally applicable, so that our words will always have the same meaning for any culture we are talking about. Then we decide the manner and the extent of each culture's response to the values chosen within each cluster, and are able to distinguish what are most important in each. It is assumed that each culture is integrated, or partially integrated, through its value-responses or value-tones.

The Harvard study is in process and many people are contributing to it. Although the first book of the project itself has still to be published, several articles present findings coming under the general project. Three of the five cultures specially chosen for study have been made the basis of a paper by E. M. Albert (1956), who gives the following values as characteristic of them: Ramah Navajos—Knowledge, family possessions, enjoyment and health; Mormons—Work, health, education, recreation; Texan Homesteaders—Individualism, personal success, community programs. Using the same kind of approach, Cora DuBois (1955) presents three focal values as those characterizing the present-day United States: effort-optimism, material welfare, conformity.

The logical foundation of the approach set forth by Kluckhohn appears indisputable. The first question is: Can we agree on what the basic clusters are and what parts under each are most important for value responses? Assuming that we can agree here on major points, we move to a more difficult question: Can we agree on what are the responses, the value-tones or value-emphases, of the actual cultures? Beyond this question is still a third one: Will the analyses of each culture so obtained be sufficiently free from complexity that effective comparisons among cultures will be clear; or—the reverse side of the same question—if we greatly condense for simplicity, as Albert and DuBois have, is the analysis accurate and does it reveal the essentials of the whole truth?

TYPES OF INTERRELATIONSHIP OF PARTS

We now return to our first and most basic differentiation of concepts. At the opposite extreme from the view of integration as dominance by a single value or group of related values is the view that integration is interrelationship of parts. And here we have to ask: What do we mean by interrelationship? Do we mean simple interrelationship or do we mean consistency of parts and balance of parts in a co-ordinated whole? In one sense, the difference is of degree rather than of kind; but the degree can be so great as to make it desirable to think of the two as usually constituting two distinct

cases. A good part of the controversy over integration of parts has arisen through confusing these cases.

There is no question but that all parts of a culture are interrelated in the first sense, as we see when we introduce even the simplest change. Nothing is completely neutral, even in physics. A stone thrown into the sea presumably has its first and greatest impact in the region where it hits, but it also sets in motion a force which finally, even though imperceptibly, affects the waves that wash on every continent and rock, reef, and island. A particular change which may seem to make no difference outside of a very small area can have very far-reaching repercussions, as did the straw that broke the camel's back and the missing nail that lost a war. But since culture is a human product, the relations among its parts are more than physical or mechanical. All parts of a culture are dependent on human choice; and not only material goods but time and energy, including the energy used for choice-making, are limited. Among the social sciences, this phenomenon has been most stressed by economics, the most elementary student of which knows that a single change in price or quantity of any good offered on the market may affect all prices and all demand. Similarly, a single choice may affect all choices. On this foundation of interrelationship of parts through competition for limited resources arises the second kind of interrelationship of parts, which includes consistency, co-ordination, complementarity (Pareto 1927: 268; Edgeworth 1925: I, 119; Hicks 1934: 69-73), and balance. A Gestalt in psychology is essentially an integration involving such qualities as these, and integration of culture, as a Gestalt, requires all of them.

In the market it has long been recognized that certain things are chosen, not merely as alternatives and complements to others, but also as offsets, antidotes, and compensations. The offset-compensation side of this aspect of integration provides a good deal of the material that interests those concerned with the psychology of the unconscious. Ordinary experience gives many examples. College professors are said to be the most avid readers of detective stories, and talk-fests are described as the favorite recreation of home-keeping women who are isolated from society for the greater part of their day. The expression "it drove him to drink" dramatizes the importance of compensation in integration; and in times of rapid change whole cultures have become demoralized because compensations for disturbances were inadequate or destructive.

Of all anthropologists, Malinowski has perhaps gone furthest in espousing the idea of culture as a functional whole. He regards it as an integral in which the various elements are interdependent, and in the second sentence quoted below goes even further (1944: 150):

Culture is a system of objects, activities and attitudes in which every part exists as a means to an end. . . . Such activities, attitudes and objects are organized around important and vital tasks into institutions. . . .

The book from which this is taken was published after Malinowski's death, and he probably would have clarified his application, had he lived. For, while all objects, activities, and attitudes relate to something, it

is too much to claim that they are all organized around important and vital tasks. Activities and attitudes—and objects too—can, as we have seen, have weakening effects and can destroy institutions without replacing them. The term "reservation culture," which is used to describe the culture of some Indian groups in the United States, typically means a culture marked, not by important and vital tasks, but by apathy and loss of sense of security. Malinowski was thinking of an abstraction which he regarded as an ideal for a culture to approximate.

Gregory Bateson (1936: 29-30) analyzes the *naven* ceremonies of the Iatmul of New Guinea from the point of view of interrelationship of parts under five main heads: structural, affective, ethological, eidological, and sociological. On the basis of this analysis, Bateson (p. 264) concludes that even though the Iatmul might not be conscious of interrelationship, he was "justified in expecting every aspect of culture to be represented in every bit of behaviour." He does not claim that he himself has originated or substantiated any general theory of interrelationship, but only that the method he used for the *naven* ceremonies is capable of broader application; and that, so far as these ceremonies are concerned, if his observations are justified the theory of the integration of parts is demonstrated.

Firth clearly states his position on the integration of culture without making the extreme claims of Malinowski. He speaks of the "dynamic interrelationship of items in a culture, each item tending to vary according to the nature of the others" (1939: 18-19).

It is possible that economists have done as much as anthropologists to spell out the meaning of integration of parts of culture, though economists have not generally used the term "culture." The primary concern of economics is choice among scarce resources, material and non-material (Robbins 1937: 16), and this is the stuff of which culture is composed. Man can choose nothing that is not a part of culture or does not become so by the act of choosing it. Nor can there be anything in culture that is not chosen by man. In particular, plans of consumption and standards of living are in themselves organizations of culture, the expressions of choices made from it or desired from it.

Simon N. Patten in 1892 referred to the laws of harmony, of diversity, and of grouping in consumption, and the fact of disharmony. From the law of harmony "it follows that the total utility of a group of commodities will be greater than the sum of the separate utilities, if the combination is harmonious, and less, if it is discordant" (1892: 56-57). He further notes that some choices (parts of a culture) are negative in their effects and can disarrange the whole.

These ideas of interdependence of parts took little hold in the main fabric of economics, however; and only in 1944, in the Presidential Address of the American Economic Association, by J. S. Davis, did the subject receive major attention. The content of living, Davis said (1945: 9), is made up of a complex of working conditions, possessions, freedoms, and "atmosphere" (an all-inclusive term), and its character depends not only on what goes into it but

in particular upon the degree of harmony or *balance* among its components. . . . Improvement . . . can often be brought about more easily and more substantially by limited changes in its structure than by increase in the consumption level as a whole. As in Liebig's law of the minimum and its variants in science and economics, a favorable change in some highly deficient component may so improve the whole content that reductions in some others will cause no injury.

At this point it may be well to clear up a confusion about the word *balance* in connection with culture—an extremely important distinction. A balanced attitude is regarded as an attitude of the middle way; a person maintains his equilibrium by avoiding any extreme. This is the sense in which Benedict used the term Apollonian to describe the Zuni. But there is another meaning of balance, and a balanced culture is one in which equilibrium is secured by differences which, for human purposes, offset or complement each other. These differences may be extremes, and a balanced culture may be one of great extremes if the directions of extremes are contrasting and complementary. This is the sense in which Davis uses the term.

The difference between the two uses of balance is clearly shown in the attitude and the culture of the Athenians of the classical period. As for attitude, they had a middle-of-the-road ideal, "balance" in Benedict's Apollonian sense. In no culture has so much been said of the principle of nothing in excess—the golden mean. But they did not have a "balanced" culture, for in their single-minded devotion to the ideal of intellectual beauty they neglected to protect their sanitation, even though the Cretan culture before them had provided an amazing demonstration of an effective urban sanitary system. The immediate cause that led to the decline of Athens and the decay of its creative spirit was a plague, which spread rapidly and could not be controlled because of the lack of sanitation in the city. Athens "chose to put Beauty before Security, to build her temples on the Acropolis rather than lay water pipes to the Piraeus" (Zimmern 1922: 299).

It is balance in this second sense with which we are concerned in a dynamic culture, and balance in this sense is never fully attained. As soon as one equilibrium is approached another is sought, and the culture is always moving from one imperfectly attained equilibrium toward another. Growth in culture is much less likely with a policy of nothing in excess than with disequilibria seeking to offset one another.

THE TWO MAIN CONCEPTS OF INTEGRATION COMBINED

It has already been said that the two most clearly differentiated concepts of integration—integration by dominance of leading idea, and integration by interrelationship of parts—are not antithetical. Some of the early sociologists, not primarily interested in integration of culture as such, appear to have held this two-fold concept of integration. Such references in sociology are as old as Comte, whose system involves the mutual connections of political institutions, manners, and ideas, and for whom this consolidated whole is in turn dependent on the state of humanity in all its aspects. Weber was concerned with both the causal correlation of the facts of culture and the necessity for understand-

ing the values that give meaning to the facts. A similar concept appears in William Graham Sumner: the folkways are subject to the strains of consistency with one another and of adaptation to the ends the society seeks.

Among anthropologists, Radcliffe-Brown's conception of integration includes both aspects. As for domination by a leading idea or ideas, he says (1933: 233-34):

(1) A society depends for its existence on the presence in the minds of its members of a certain system of sentiments by which the conduct of the individual is regulated in conformity with the needs of the society.

(2) Every feature of the social system itself and every event or object that in any way affects the well-being or the cohesion of the society becomes an object of this system of sentiments.

On integration of parts he has this comment:

Every custom and belief of a primitive society plays some determinate part in the social life of the community, just as every organ of a living body plays some part in the general life of the organism. The mass of institutions, customs and beliefs forms a single whole or system that determines the life of the society, and the life of a society is not less real, or less subject to natural laws, than the life of an organism (pp. 229-30).

In economics, the same two-fold concept is held by Hoyt. The two parts are clearly stated as belonging to the same whole in her definition previously referred to (1928: 9); and the interrelationship aspect is enlarged upon in her discussion of "organic unity" in a later work (1938: Ch. 23).

Mannheim's concept of *Weltanschauung*, discussed earlier, in part belongs here also, since to him *Weltanschauung* was both a system of sentiments and, much more important, a way of living.

DEGREE OF INTEGRATION WITHIN THE TYPE

But now it is high time to make a distinction within the concept of integration in terms of degree of integration. This has been implied earlier, but was not made specific. Whereas it may be true to say that all parts of a culture are integrated or at least approach integration, in the over-all sense of dominating idea as well as in the sense of interaction of parts, it is by no means true that all parts are equally integrated—or perhaps it would be more correct to say that some parts, by their nature, stand more on their own feet without depending on culture pattern, than do others. This distinction in terms of degree is shown most clearly in circumstances of culture change. A few kinds of change can occur with no appearance of cultural shock at all, especially when they are introduced to young persons, whose culture patterns are not yet deeply established.

In Keesing's discussion of culture change (1953: 82-84), he says:

One of the most tenuous yet potentially important fields of study concerns . . . the loci or focal points of persistence, conservatism and stability, on the one hand, in culture and personality, and change and mobility on the other. . . .

Perhaps the most ambitious attempt to collect and systematize the data involved is by Keesing. By tabulating loci of

persistence and of mobility as shown in a number of acculturation studies, he develops a broad framework of hypotheses in relation to each of these facets of dynamics. First are set out those zones in culture which appear to show a high frequency of persistence, as pertaining to basic survival, security, integrity, value, problem solving, for the group concerned, and in which, if change or interference occurs, the greatest disturbance and tension is likely to be generated. Categories cover essentials of psychosomatic conditioning, communication, organic maintenance, primary group relations, prestige status maintenance, territorial security, and ideological (including religious) security. By contrast, zones in which mobility or ready change tend to appear with high frequency are nonaffectively charged techniques summarized as "instrumentalities" (e.g., tools, etiquette, military tactics); voluntary elements of taste and self-expression, achieved status systems, and other elective or competitive types of behavior, and more impersonal or mass structures. . . .

Such materials on rates and loci of change and their determinants are cited here as broad leads. . . . They illustrate the extent to which cultural studies offer practically virgin ground in many respects when dynamic problems are posed.

The matter has been of concern to at least one economist (Hoyt 1956: 14-15), who has approached it from the point of view of specific categories of material goods in the standard of living to which least cultural resistance is offered.

The first new choices in general fall into four categories, of which the fourth to some extent crosscuts the other three. The first of these categories is goods making an immediate appeal to sensory tastes which are universal or nearly universal among all peoples, such as sweets of all kinds . . . , tobacco and drugs which give temporary emotional satisfaction, and intoxicants. . . . Almost everywhere the per capita consumption of sugar notably increases as a new money economy comes in.

The second category, in some cases related to the first, is proprietary medicines and devices to which magical or semi-magical qualities are attributed. . . . A considerable part of the advertising in vernacular papers published among underdeveloped peoples is of proprietary drugs and devices. . . .

The third category is of goods and services associated with prestige. Among foods, white flour and white-flour products generally have more prestige than the old familiar staples. Very often the most prestige-carrying goods are something to wear. . . . That beads, metal decorations and bright cloth should be a part of the equipment of traders going to far-flung tropical shores is taken for granted. . . .

Superficial fashions flit over under-developed peoples in very much the same way they flit over us, without necessarily leaving any permanent marks at all; for the people who responded warmly to blue beads at the first visit of the trader may not show a favorable reaction to that particular item the next time it is brought.

The fourth category, to some extent crosscutting the first three, is that called "targets" by Major St. J. Orde-Browne, who says of Africans (1933: 35):

astonishing differences will be found to exist between tribes. Cooking pots, hurricane lamps, shoes, umbrellas, hoes and numerous other items, will each be found to be the particular favourite of a certain community.

There seems to be no particular logic about targets except that they are something which can be added on to the existing culture. The glamor of their immediate

attractiveness may take precedence over their relationship to other things and sometimes over their practical usefulness (dark glasses to wear after sundown, for example). They may lead to other culture changes, but they do not require them. When a person has once hit his target, he may seek several more of the same target (as a dozen pairs of shoes) or he may sit back and rest, perhaps for the remainder of his life.

This situation has very important and practical application in modern economic development policy. The fact that we do have certain cases of slight resistance to change has led to serious mistakes by business men attempting to develop "backward" areas, and to grave errors by economists in dealing with the subject of economic development. They have witnessed ready acceptance of a few new goods from our own society, and have assumed that it was "natural" for such goods to be desired and for people to be willing to work to get them (Greaves 1935: Ch. 4). Whole volumes dealing with economic development in under-developed areas today do not say a word about culture pattern or incentive, but assume that if capital and technology are made available in these areas the responses of potential wage-earners to the possibilities of expanding consumption will be the same as they are with us.

At the other extreme from these categories of material goods are elements of culture which, if disturbed, offer very deep resistance.

While the change from one culture to another is never a smooth and even process, integration with the new sometimes occurs by more or less regular steps and at other times may involve one major step or leap. The latter phenomenon has been noted by Sol Tax among the highland Indians of Guatemala. These Indians live in, or on the edges of, a Ladino culture. Some small changes take place quite readily, but no major changes occur until Indians are ready to assume Ladino culture altogether. In many cultures, dress is changed quite easily; but not among the Guatemala Indians. The Indian does not begin to wear Ladino shoes or other articles of clothing even though he can afford to do so. Clothing and housing are symbols to him—important symbols because they are particularly visible—and will not be changed until he is willing to step over to the new as a whole. Tax (1958: 231-32) says:

A change in consumption pattern that is interpreted as signalling a change in group identification can and will be made only when the major shift in identification is already accomplished. . . . In a deeply cross-cultural situation a change in social identification means crossing a chasm that is deep and wide; the Indian who has become a Ladino is transformed into a different person in way of thought as well as way of life.

APPROACHES TO THE CONCEPT OF INTEGRATION FROM THE POINT OF VIEW OF ITS FUNCTIONAL APPLICATIONS

There are two somewhat related approaches to integration which, by attempting to show how the concept may be useful in practical life, lead to an analysis or a partial analysis of the concept itself. The first approach, and the more concrete but usually the more limited in its scope, is that of mental hygienists who have observed

that disturbances in psychic and mental health may be traced to lack of personal integration and, beyond this, to lack of cultural integration. This approach, in other words, is by way of seeking an explanation for an amelioration of a condition of imbalance, which leads to the necessity of understanding what integration itself is.

The World Federation of Mental Health has prepared for UNESCO a volume dealing with the nature of conflicts arising when modern technological culture impinges on the culture of "under-developed" peoples. Mead (1953: 9, 10) says:

The approach of this survey is based on the recognition that a culture is a systematic and integrated whole . . . the assumption itself drawn from field work among many kinds of societies that a change in any one part of the culture will be accompanied by changes in other parts, and that only by relating any planned detail of change to the central values of the culture is it possible to provide for the repercussions which will occur in other aspects of life.

At the end of the book appear seven recommendations, based on study of actual cultures in transition, for easing the process of transition.

Mental hygienists, and anthropologists concerned with mental hygiene, have clearly shown that in major culture change, integration involves or may involve deep disturbances in the unconscious; and that ordinary observations may give a deceptive impression of what is going on. Therefore a professional approach, both for individuals (Weisskopf-Joelson 1953: 601-04) and for whole groups (Spindler 1957: 151-52; Hallowell 1955: Chs. 3, 19, 20), may be necessary to understand what is below the surface.

To the other "practical" approach, which is broader but looser than the first, both philosophers and economists contribute. G. E. Moore in *Principia Ethica* (1903: 27-36) used the term "organic unities" for entities which in interrelationship have great value, though in isolation the parts may have little or no value. We have here an approach to ethics through integration, and one which is not based on value judgment. A. Macbeath (1951: 398) presents as a goal the extent to which men reach a cultural state involving

complete mutual congruity between the spirit of which it is the outward manifestation, the type of character which it fosters, the system of activities in which it finds expression, the rules of conduct which it implies, and the ends or states of affairs to which it gives rise.

In setting forth a criterion for ethics which is not based on value judgment, Macbeath's idea resembles that of R. B. Perry (1909), for whom moral good is "most good," or a state of living in which the parts, by being harmonious, mutually support and strengthen one another.

This kind of approach to ethics, however, does not in itself help us to discover how and how far the parts are actually contributing to the desired end. Such an effort has been made in economics, in two ways, both of which go beyond the ethical position stated above.

The first way is through the concept of standard of living. Davis' discussion of the "organic" nature of standard of living has been referred to. We assume that nothing in the standard of living is completely neutral;

some parts have much more open-minded effects than others, but every part has some effect on other parts and on the whole. From one point of view we can conveniently class these reactions (according to their degree) as protective, expansive, or destructive, even though in a specific situation we are far from being able to identify all the parts by such terms. The nature and degree of reaction in certain specific cases can be learned, in some small measure, through application of the sciences of nutrition and health, including mental health. The relation of food to strength for all else is perfectly obvious; the reaction of excesses, when they can be defined, is obvious too. Housing is an area of the living standard that has important reactions on other areas: the space provided by larger houses encourages the use of some things that would not otherwise be used. The effect of parts of standard of living on the development of the whole in general, and as illustrated in Jamaica, is discussed by Hoyt (1960: 134-36). In general, however, our knowledge of interaction is inexact, and we can only hope for a sympathetic approach to situations of interaction and for objectivity in evaluating knowledge that has been or is to be attained. But in the reaction of parts within the standard of living we have at least a beginning.

The second way is that of so-called welfare economics, the underlying objective of which is to show what choice and arrangement of parts will bring about maximum returns for a given purpose, assumed in the final analysis to be welfare. "Welfare economics is normative, but given its norm, the determination of the policies to be undertaken in accordance with it is a strictly scientific enterprise" (Reder 1947: 14).

Though economics ordinarily concerns itself with money inputs and outputs, its ultimate concern is with everything that is scarce, and thus in theory it embraces the whole range of culture, except possibly language. Its significance consists in its help toward enabling us "to choose with full awareness of the implications of what we are choosing"; and so to choose with consistency (Robbins 1937: 152). Most of the recent work in welfare economics has been in graphically or mathematically expressing economic relationships of all kinds, not simply in equilibrium but in reference to increasing and decreasing well-being. All relationships can be so expressed if the graph or statement is complex enough. The exactitude of the method is logically satisfactory, but in practice welfare economics becomes involved in a confusion of unknowns, and so the graphical-mathematical representation is thus far of very limited use for anything but method.

SUMMARY

All the interests in the integration of culture and all the approaches to the understanding of it which we have considered are imperfect, and the totality they give us is incomplete. But each discipline has opened a door to it, though the variety of doors is at first bewildering. Together these approaches make plain, as no one approach could, how powerfully the imagination of man has been seized by the concept of integration of culture; and how necessary the concept is to understanding the nature of other cultures, and the issues which we confront in our own.

Comments

By DAVID F. ABERLE ☆

Elizabeth Hoyt's article deals with various meanings of the phrase "integration of culture." Although Hoyt specifies many senses of the term integration, her second term, culture, remains undefined. Yet surely it is as important to say what is meant by culture as it is to say what is meant by integration. Integration refers to "some congruity of the parts" of culture (p. 407), but it seems difficult to say what the nature of the congruity is without saying what the parts are, and what the whole is. Congruity is one thing for a jigsaw puzzle, another for a bridge, still another for a symphony, and yet another for an organism or a machine. Even if there are general terms which cover types of congruence for all these phenomena, their specific referents will be sufficiently different to make a difference in the concrete application of such general terms.

As I tease out the implicit meaning which the term culture seems to hold for Hoyt, it appears to me that she has taken the view that culture is shared behavior, attitudes, and values. For her, the relationships that the parts of a cultural system have to each other seem to be a matter of their congruence from the point of view of a typical individual in a social group with a common culture. This conclusion I adduce from her arguments and her examples. She says that the goal of the integration of culture "is that ultimate energies should be conserved and that institutions and values should . . . be consistent with common ends" (p. 407; italics supplied). The interest in common ends suggests the "shared values" view of culture. She goes on to say that the integration of culture "depends upon *psychology* for interpretation of the nature and process of the interrelation of parts" (p. 407; italics supplied). She says that Wissler succeeded only imperfectly in explaining cultural integration by reference to association psychology, but implies that Gestalt psychology would have been more useful (p. 408).

Although Hoyt says that "today . . . integration is more often discussed in terms of the function performed, rather than of associations" (p. 408), her development of the theme of functional integration seems again to involve *Weltanschauung* and values. Thus, within what Hoyt sees as the functional position, she distinguishes two approaches: on the one hand, analysis in terms of a dominant idea or value, or a cluster of similar dominant ideas or values which control the rest of culture (the rest of the ideas or values?); and on the other hand, analysis in terms of "in-

teraction and inter-coordination among many similar and dissimilar traits, factors, interests, or values" (p. 408). The dominant value approach she compares with what Freedman and others (1956) term normative integration, and the interaction approach with what they call functional integration. Hoyt considers the two approaches to be related, rather than contradictory, and quotes Mannheim at length as showing how they are related. The quotation deals entirely with *Weltanschauung*: Mannheim claims that attention only to the theoretical philosophy of a culture is insufficient—*Weltanschauung* is to be found in a variety of expressive and artistic phenomena as well. (This passage, by the way, shows extensive parallels with Boas' [1911] views in the "Introduction" to the *Handbook of American Indian languages*.)

To anticipate my later argument somewhat, it is evident that to Hoyt one approach to the integration of culture is through the analysis of dominant values. Thus far, it is not completely clear what sorts of parts she considers under the heading of functional integration. The work of Freedman *et al.* is not available to me as I write this, but if memory serves, what they mean by functional integration is approximately what Durkheim (1947) meant by organic solidarity: the interdependency of parts of a society, which derives from specialization of function.

Thus, to take a simple example, for Freedman *et al.*, dairy farmers and city-dwellers would be in a state of interdependency not because they share common values, but because their requirements are complementary: customers and milk, respectively. It therefore becomes of interest to see what Hoyt deals with in connection with interrelations of parts. Throughout her treatment of this topic there is a definite tendency to turn again to values, and not to the sorts of parts in which Freedman *et al.* or Durkheim were interested. She begins by describing approaches which "search for sets of values that will characterize a culture when they are taken as a whole," even though no one dominant theme emerges in the set (p. 410). "It is assumed that each culture is integrated, or partly integrated, through its value-responses or value-tones" (p. 410).

Hoyt continues her development of interrelationship along similar lines. Thus one view of interrelationships, she says, is simply that the parts are in a relationship such that a change in one part implies a change in another. This type of interrelationship she sees as resulting from the fact that "all parts of a culture are dependent on human choice

. . ." and she uses market economics as an instance of this sort of interrelationship (p. 411).¹ Thus here, too, we find a relationship of parts which are values, attitudes, motives, ends, etc.

There are, however, she points out, more complex meanings of interrelationship: consistency, co-ordination, complementarity, and balance. Here too, the consistency, co-ordination, etc., seems to inhere in individuals taken singly or *en masse*. Thus, as an instance of complementarity, she tells us that women who are alone much of the time like to gossip when they do get together. Similarly, there was a defect of balance in Greek culture, which seems to be a matter of value balance: the Greeks were interested in beauty, but did not have a balancing interest in sanitation.

There is a further section on integration by dominance and by interrelations of parts which once again asserts that the two types of integration are not antithetical; but the statements made in support of this view are sufficiently general so that I am not sure whether the "parts" are such as she has been discussing, or such as I hope to discuss.

The article continues with further concern with integration for the individual. Hoyt discusses degree of integration, pointing out that when cultures change, "a few kinds of change can occur with no appearance of cultural shock at all, especially when they are introduced to young persons . . ." (p. 412). It seems evident, then, that lack of integration is lack of felt congruence. A further discussion of rates and locus of change again refers to individual choices, and to choices in the market.

When we turn to the "functional applications" of the concept of integration, the first example is from the field of mental hygiene: mental disturbance

¹ See Anderson (1960) for a discussion of "choice" in cultural, rather than psychological terms. He measures degree of integration by reference to what he calls "reduction of variants," or elimination of alternatives or options. He analyzes the Plains Indian Sun Dance in these terms, concluding (1960: 57):

The old Arapaho and Cheyenne dances stand in contrast with the younger ones of the Comanche and Ute. The former exhibit a relatively small number of alternatives or variants at specific points, the latter a relatively large number. Considered as process, this means that integration (Kroeber's "accommodation of discrete parts . . . into a more or less workable fit") proceeds, at least in one of its aspects, through the progressive reduction of variants.

I have not tried to bring his argument to bear in discussing Hoyt's paper, both because my comments focus primarily on the nature, rather than the degree of integration, and because I am not certain how his technique should be applied to complex cultures and to market situations.

may be related to "lack of personal integration, and beyond this to lack of cultural integration" (p. 414). There are some concluding remarks on an approach to ethics and to welfare economics via integration, where I shall not try to state the issues at stake.

What emerges from the examples and the argument is that Hoyt sees culture as a matter of shared behavior, attitudes, and values, and sees the integration of the parts of a culture entirely, or almost entirely, as a matter of the interrelationships among the attitudes, values, and activities of individuals or categories of individuals. The model of culture she has used is what I have called the linguistic model (Aberle 1960), common in early culture-and-personality writings and by no means uncommon in the writings of other anthropologists. Evidently it is a model which is now spreading to economics. The fundamental argument against Hoyt's view as I understand it is that this sort of approach to culture is fatal, particularly for understanding the sort of cultural integration in which she is interested.

The alternative approach which I shall suggest is evidently borrowed from the work of Leslie A. White, although he cannot be held responsible for my effort to use it (cf. esp. White 1949, 1959). Culture is the realm of things and events dependent on symboling. This realm includes technology, social organization, and ideology. Culture has both temporal and spatial extension. It may be conceived of as a flow, as an assemblage, or as a system, as dictated by the problem at hand. Certain convenient pieces of this realm, limited in time and in space, may be referred to as cultures. These are the cultural apparatus of specific groups or sets of interrelated groups located in time and space, as when we speak of Hopi culture, American culture, Western European culture, etc. Whatever else it is, culture is man's non-genetic device for appropriating energy from the natural environment, and a culture is the non-genetic device of a given group or set of groups for appropriating energy from the natural environment and from other cultures. For purposes of analysis, we may regard the cultures which surround and impinge on any given culture as part of its environment. By the same token, if we wish to examine one of these environing cultures, we may regard the first culture as a part of its environment. Or, as Hoyt suggests (p. 414), a symbiotic view may be taken.

A culture may be viewed as a system, insofar as, to some unspecified degree, a change in one part implies changes in other parts. The study of the integra-

tion of these parts is simply the study of the nature and degree of their interdependence, whether this involves consistency, complementarity, conflict, balance, positive feedback, negative feedback, or what have you. (Thus far, I am in agreement with some of Hoyt's most general statements about integration.) There are many ways to divide and subdivide parts. I have suggested one way: a major subdivision into the technical, social organizational, and ideological spheres of a culture, each of which is susceptible of further subdivision. The technical sphere consists of tools and their immediate techniques of application; the organizational sphere consists of relationships between social positions; the ideological sphere consists residually of ideas, values, attitudes, beliefs, motives, etc., insofar as these are not already described by reference to the first two spheres. Further subdivisions depend upon the complexity of the cultural system under consideration. Thus there may or may not be specialized tools for warfare; there may or may not be specialized economic or political units; there may or may not be ideas pertaining to nuclear physics and human genetics.

In this perspective the significant thing about a culture is not the sharing of beliefs, attitudes, and values. Indeed, even in the simplest systems, there is differentiation of all of these, because of the rudimentary divisions of social position known in all human societies: those based on sex, age, and personal characteristics. Even the integration of the ideological sphere of a culture—or the study of the integration of this sphere—involves not merely the congruence of values, beliefs, or *Weltanschauung* for individuals in any given category, but also complementarity (or conflict) of *Weltanschauungen* for individuals in different categories. Such complementarity in a simple society is nicely illustrated in Kardiner's analysis of masculine and feminine outlooks in Comanche culture (Kardiner 1945: 81–100). The degree of complementarity, congruence, balance, etc., in the *Weltanschauung* of a class of individuals, and the relationships among the *Weltanschauungen* of different segments of the population, again a matter of integration, are best studied by approaching the total cultural system in its setting, and understanding the relationships between the technical, social, and ideological spheres before trying to understand the relationship among ideological elements.²

²Some minimum sharing of beliefs, values, ends, attitudes, etc., is to be expected in any society (Aberle, Cohen, Davis, Levy, and Sutton 1950), but the diversification of all of these accompanies social differentiation (Aberle 1950).

Let me supply a brief example. It is less than ideal, since the approach I have suggested requires a more extensive exposition than is possible here. According to descriptions of traditional Navaho culture, Navahos seem to have had considerable interest in the acquisition of property, and specifically of livestock, jewellery, and buckskin, as well as a strong emphasis on generosity and a deep distrust of wealth—so deep that the wealthy were sometimes accused of having acquired goods by witchcraft. Is this an instance of value conflict, or of balance (that is, they want wealth but they are afraid of it), or of complementarity (that is, their desire for accumulation is balanced by a desire for dispersion), or simply of malintegration? From a knowledge of only these values and beliefs we cannot be sure, but there is additional information. From *Son of Old Man Hat* (Dyk 1938: 75–81) we learn that there are conflicts involved: Old Man Hat tells his nephew that people say you should not be stingy; but, says the old man, it is best to be rich first and generous later. And he dies accused of witchcraft. There is parallel information in other sources. But when we know that this value conflict exists, what *do* we know? The conflict can be shown to "govern" Navaho behavior, inasmuch as any case of accumulation can be said to represent one value, and any instance of generosity the other. We are not, however, in any position to say which one will be ascendant in any particular instance, nor are we able to say which value might in time gain ascendancy over the other. (There is evidence that today accumulation, rather than generosity, is ascendant.)

Let us turn to another set of facts. The Navahos live in a region where stock-raising is possible, and where agriculture can be practiced only in some areas, with variable success. They received livestock from the Spaniards, directly and indirectly. This made possible the accumulation of goods of great utility, for food, transportation, clothing, bedding, and so on. The success of different individuals in accumulating goods varied, and varies, because of prudence, foresight, local climatic conditions, etc. Such differences of accumulation do not operate significantly as regards food crops, where the surplus is likely to be small. At the same time, for reasons which cannot be developed here, Navaho society remained organized at a tribal level. Leadership, though to some degree hereditary, had little real power, and the adjustment of conflict occurred through self-help, as in feuds, and through compensation. A man depended on his kin for help in these matters—primarily on his matri-

lineal kin, but also on his paternal and affinal kin. Furthermore, resources fluctuated greatly: crops or herds could be wiped out by adverse circumstances, and a man then depended on his kin for economic help as well.

The ethic requirement for this situation, in which kinship relationships were the main resource in disputes and for economic aid, was one of reciprocity and mutual generosity, whereas the existence of livestock permitted sizeable accumulation of goods and differentiation of the condition of kinsmen, temporary or long-term. The material advantages of this accumulation were considerable: a man with plenty of stock ate better; could use his stock to accumulate other goods, such as jewelry and buckskin; and by a rather limited generosity could attach his poorer kin to him and be advantaged in intergroup conflict. On the other hand, by reserving wealth to himself, he ran counter to the ethical relationships required by fluctuating resources and tribal political structure: he was no longer merely one of a group of sharing kinsmen. There was, then, a conflict between accumulation and dispersion of wealth, which is evident in Navaho values, and which was solved by a series of leveling devices, ideological and other. Some dispersion of wealth was secured because wealthy men were expected to have expensive ceremonies when they fell ill. This dispersion was particularly likely to occur in old age, with declining health, and might result in the loss of most of a man's livestock. In addition, inheritance patterns did not concentrate a man's holdings in a single heir but dispersed it through his matrilineal group, so that accumulation over the generations was difficult if not impossible. Fear of the witchcraft of others and fear of accusations of witchcraft also led to generosity. In this way, the accumulation permitted by the technology and the leveling required by the social organization were kept in balance. In this perspective, witchcraft beliefs, ceremonial practice, technology, and socio-political organization can be brought into "congruent" relationships, whereas otherwise we have merely items of ideology in uncertain relationship. (Similar interpretations can be found in Kluckhohn 1944: 62-64 and Aberle 1961; supporting materials can be found in these sources and in Hobson 1954. Full documentation would require an extensive bibliography.)

A stronger emphasis on accumulation and a weaker one on generosity have followed Navaho engagement in market relationships and control of law and order by a body of police imposed on the Navaho by the Federal Govern-

ment. Market relationships have given rise to new ways of using wealth, and law and order to a lapse in one important area of functioning of kinship units. In addition, government-imposed stock regulations have reduced opportunities for livestock accumulation and weakened economic interdependencies among kinsmen. Any predictions about the future ascendancy of one or the other value rest on predictions about Navaho economic and political conditions, and not upon the congruence or conflict of Navaho values in the economic area.

Even if the example has not been analyzed in a fully systematic fashion, it does supply an instance of the integration of technology, social organization, and ideology, and furthermore an instance of the integration of elements of ideology when they are seen in the context of the rest of the parts of a cultural system.

I am pleading, then, for a broader conception of culture—and a different one—than is to be found implicit in Hoyt's article and explicit or implicit in the work of many anthropologists as well. Furthermore, in order to discuss integration of parts, more must be said about what the parts are than is to be found in Hoyt's presentation. Finally, analysis of the integration of values, either by attention to dominant themes or to smaller clusters of values, should follow analysis of the technical and social spheres, rather than precede it. The same is true for *Weltanschauung*, motives, ends, and ethos, in their various overlapping and distinct senses.

[Manchester, England, 10.4.61]

By HARUMI BEFU*

Among the contributions which Hoyt makes to anthropology in this admirable article are two. First, she brings together various conceptions of integration and gives them order by classifying them under a limited number of headings. Second, she draws our attention to the ideas of those scholars who are apt to be forgotten or to go unnoticed by the average anthropologist, but who nevertheless have significantly contributed to our understanding of the nature of integration of culture.

Admirable though Hoyt's paper is, its title is an unfortunate misnomer. It falls short of providing a comprehensive coverage of the topic or an extensive bibliography, requirements of any review article. (See inside cover of CA for the editor's definition of a "review article.") Hoyt even overlooks many of the standard sources, and consequently bypasses numerous problems and topics in the field of integration of culture

which are provocative of extensive as well as worthwhile discussion. The list she omits and the area she neglects to cover are too vast to be handled adequately here. Some of the concepts which in one way or another have direct bearing on the concept of integration are: Linton's "core" and "interest" (Linton 1936: 347-66, 422-63); Redfield's "great and little traditions" (Redfield 1956); Steward's "levels of sociocultural integration" (Steward 1955: 43-63); F. R. Kluckhohn's "dominant and variant (substitute) value orientation" (Kluckhohn 1950, 1953); Parsons' "value orientation" in the social system (Parsons 1951); Opler's "theme" (Opler 1945); Kroeber's "total culture pattern" (Kroeber 1948: 316-18) and "Oikoumenē" (Kroeber 1952: 379-95); Herskovits' "focus" (Herskovits 1949: 542-60); Bidney's "teleofunctional," "psychological," and other forms of integration (Bidney 1953: 366-99); and Kroeber's and Nadel's "ethos" and "eidos" (Kroeber 1948: 292-95; Nadel 1953: 395-402). Any "major survey of knowledge" on the integration of culture should review these and probably other concepts that are well known to anthropologists, discuss their relationships with each other, and show what relevance they may have to Hoyt's own conception of integration. At the very least, she should have indicated why she eliminated such obvious sources as these from her article. In the following paragraphs, I should like to comment on some aspects of Hoyt's paper.

1. Hoyt classifies concepts of integration into two main types: "integration by dominance of leading idea"; and "integration by interrelationship of parts"; these two types, she maintains, are not antithetical. What she does with "integration by parts as such," exemplified by the Harvard study, is not clear. The label might prompt one to include it in the latter of the two main concepts, viz., integration by interrelationship of parts. But this does not seem wise, since integration by a dominant idea and integration by several values—the Harvard approach—are antithetical by Hoyt's own admission. For was it not "the impossibility of characterizing every culture by a dominant value" which "led to a search for sets of values that will characterize a culture?" Although, as another alternative, integration by parts as such could be given an independent status, thus recognizing three, instead of two, main concepts of integration, this does not seem to have any particular advantage; besides, this approach would necessitate accounting for the relationship of this last main concept to the original two concepts. A

more parsimonious approach probably is to group together integration by a dominant value and integration by sets of values under a single heading, such as "normative integration," as against integration by interrelationship of parts, which might simply be labeled "non-normative integration." This mode of classification would be consistent with Hoyt's dictum that the two main concepts of integration are not antithetical and yet at the same time retain some degree of simplicity.

2. The two concepts of integration, normative and non-normative, are not only not antithetical but are probably both necessary for the on-going of a culture. Benedict's own concept should be regarded as one which combines the two. Her concern with integration by interrelationship of parts is clearly indicated in her discussion on the lack of cultural integration (1934: 206-13). Parsons (1951) may be cited as a prominent example of combining a set of values, rather than one dominant idea, with (non-normative) social institutions.

3. "The primary concern of economics," Hoyt tells us, "is choice among scarce resources, material and non-material." Since "all parts of a culture are dependent on human choice," and since there cannot "be anything in culture that is not chosen by man," we are led to believe that economists are particularly suited to analyze culture. There are, we are told, categories of material goods which people are ready to adopt, and also "elements of culture which, if disturbed, offer very deep resistance." Thus, the degree to which different parts of a culture are integrated, if I am not misinterpreting Hoyt, can be studied on the basis of choices which people make.

Basic to this economist's approach is the concept of choice. I seriously question the utility of the assumption—and it is an assumption—that everything cultural is chosen by man. Patrilineal descent, belief in witchcraft, silent barter, and communism are certainly all "parts" of culture—at least in some sense of the term. But we can hardly say that they were "chosen by man"—at least in the same sense as one might consciously choose sweets or medicine. A society may cease to practice matrilineal descent and, anthropologically speaking, adopt bilateral descent. But the utility of explaining this process in terms of human choice is highly dubious. At least, some other approaches, e.g. Murdock's (1949), seem to be more fruitful.

There is a school of thought in anthropology (White 1949) which espouses the view that human choice can and should be completely eliminated from the analysis of cultural phe-

nomena. Even if one sets aside this extreme view, there should be some limits to the area in which the concept of human choice is to be used meaningfully in explaining cultural processes.

One reason Hoyt does not delimit the area in which the economic approach on the basis of human choice is to be applied may lie in her concept of "parts of culture." Without defining the term, she seems to use it to denote anything cultural, from material goods to values. No doubt, values, kinship terms, money, sweets, medicine, and prestigious services are all cultural products, and hence parts of culture; but there is also little doubt that a good many of these "parts," and changes therein, are not controlled by simple human choice in the way that sweets or medicine are. Hoyt's discussion does not convincingly demonstrate the utility of treating all these heterogeneous kinds of "parts of culture" as if they are uniformly amenable to the economic analysis of choice.

[*Madison, Wisconsin*, 25.3.61]

By AUDREY BUTT☆

This is a useful survey of the various theoretical approaches which have been applied to the study of integration of culture. I certainly support Hoyt's view that, although each method has made a contribution to a deeper understanding of culture, all attempts at analysis with a view to ascertaining the degree and character of culture integration have so far been most unsatisfactory. As no one method has given us an exact idea of the nature of cultures and the essential interrelationships in cultures, it is obviously unwise to use any single approach to the exclusion or non-consideration of others.

To try to assess the degree of integration and the nature of a "static" culture at a particular moment, using principally the two major concepts of "integration by dominance" and "integration by parts," necessarily places too great a reliance either on the research worker's subjective impressions, or on inadequate literary accounts. It encourages the overenthusiastic theorist, with a bee in his bonnet, to juggle the data. It also makes for high-sounding generalities, useless either for descriptive purposes or for a better understanding of the peoples being studied.

For those investigating the nature of integration of any particular culture, studies of culture contact and culture change are more rewarding. These studies can be done by selecting for investigation a culture which has at some previous date been competently described; by making a return visit to one's own previous field of research; or by electing to visit a society which is undergoing rapid changes. A record

and analysis of the effects and repercussions of elements introduced from outside contact, or of modifications due to the course of changes within the culture, provide the necessary conditions for testing and checking hypotheses concerning essential relationships. Moreover, studies of culture contact and culture change, through the very fact of change, emphasize values and variations in values held by the people of the society, thus freeing the anthropologist from the necessity of relying entirely on the values of his own cultural background as a sole reference and basis for judgment. In other words, such types of investigation alone enable the researcher to achieve a more scientific detachment and objectivity.

It is useful to assume that there is a pattern of society and culture, and that there is some degree of integration in the culture being studied. It is also useful to describe and analyse any obvious interrelationships and consistencies found in a people's concepts of thought. Nevertheless, I question the value of the concept of integration of culture for any profound understanding of the nature of cultures in general. The differences among cultures are immense; the changes, variations, and combinations of features are infinite in number. Can any scientifically valid or useful conclusions about culture integration be achieved, therefore, beyond those which are most general? In England the functionalists themselves appear to have had similar doubts, for by 1937 Radcliffe-Brown had drawn a definite distinction between the study of social systems and culture. This distinction he expressed as follows (1958: xvi):

You cannot have a science of culture. You can study culture only as a characteristic of a social system. Therefore, if you are going to have a science, it must be a science of social systems.

A comparative study of religious systems, systems of etiquette, etc., is probably far more rewarding for those who wish to understand the nature of cultures. For example, analyses of rain-making ritual and cultivation in Pueblo culture, of social values and pastoral activity among the West African Nilotes, of totemism, art, and hunting and collecting among the Australian Aborigines, give a much more valuable idea of types of interconnections between economies and culture and society in general than any attempt to determine to what degree, in the fields of economy, ritual, and values, these societies may be said to be integrated.

[*Oxford*, 18.3.61]

By MORTON H. FRIED☆

Though reviews of the concept of cultural integration may be found in

various textbooks of anthropology, for example Herskovits (1949: 214-26) and Honigmann (1959: 131-35), the importance of the theme is such that it deserves much more complete and elaborate treatment than it has yet received. Hoyt is to be commended for broaching the subject. Unfortunately, her treatment is less than might be desired, even considering the limitations of space. Since much of what follows is merely critical, I realize that I am laying myself open to the same objection. Nevertheless, I am grateful for the opportunity of contributing a few remarks which may broaden the discussion which Hoyt has hopefully reopened.

Perhaps the first thing that struck me in reading Hoyt's article was the failure to give either a systematic sketch of the history of the idea or a sufficiently catholic summary of the opinions which have been advanced under the rubric of integration. It is my impression that a general, though perhaps at times inchoate, feeling of integration was expressed by writers dealing with ethnographic topics long before this field of knowledge was formalized. Certainly Ibn Khaldun had such ideas, and they are suggested later by figures of the 18th century like Adam Ferguson. By the end of the 19th century, thoughts about the integration of culture had proceeded quite far. In one sense they seem to have outrun their theoretical base, since the epistemologically prior concept of a culture was only poorly developed. On the other hand they related to the advances in nationalism so striking at the time. Certainly the advances made in the discussion of cultural integration in the 20th century are an aspect of the concern with particular cultures. Even those earlier anthropologists who, like Morgan, had intimate knowledge of particular aboriginal cultures, invariably generalized their formulations to culture at large. This did not mean that they were not aware of problems of integration; but they phrased them differently. Morgan, for example, assumed that culture on any given level was more or less in harmony as to its component parts. This is clearly a fundamental methodological assumption in *Ancient Society*, but it is even clearer in his treatment of the theme of hospitality in American Indian life and in his view of the interdependence of such factors as mode of subsistence, social organization, and house type (Morgan 1881).

It is well to pause here and note that the direction of the inquiry into the development of the concept of cultural integration suggested in the last paragraph does not parallel that taken by Hoyt. She has concentrated, it seems to me, on one particular kind of specula-

tion about integration, that explicitly concerned with values, with the ideological expression of cultural *Geist*. In performing this task, however, her review is incomplete on the anthropological side. We miss such formulations as Herskovits' "focus," Morris Opler's "themes," and Hoebel's "postulates." We miss a discussion of relevant Kroeberian concepts of "configuration" and "climax," and some references to the knotty problem of style. We even miss a discussion of Durkheim and his seminal role in the development of the functionalism that so clearly underlies later social anthropology.

When Hoyt divides concepts of integration into two categories, one of which includes reliance on one theme or upon a dominant theme, and the other seeing integration as approaching the equilibrium of disparate but mutually interacting elements, she is missing something of great importance. Both of her categories are essentially one; as indicated above, both are concerned with ideological manifestations of cultural systems. What is neglected is the non-ideological—the integration of institutions apart from the rationalizations of the culture carriers. It is in this realm that 19th-century anthropology made substantial contributions: Tylor with his idea of "adhesions" and its obvious, if only partial, implications of integration; Morgan for his assumption that key inventions set the stage for major regroupings of cultural life; and Marx and Engels [though not anthropologists!] for their assumption that cultures are precipitated about a core of technological and economic factors. It is curious that the paired concepts of core and superstructure, whose implications for cultural integration have been discussed for some years and by different anthropologists, are totally neglected by Hoyt in favor of the pattern approach. In this regard, too, it would have been of greater utility to cite an anthropologist's criticisms of Benedict's work, rather than those of MacBeath, a Professor of Logic and Metaphysics.

A fascinating aspect of Hoyt's article is its calm and completely confident assumption of two highly controversial theses. The first is that all culture can be understood and interpreted along the lines of the models constructed for explaining capitalist economies of the 19th century. The second is that all cultural change is ultimately a matter of human choice. The second assumption, despite its logical priority, is historically dependent on the first and we are indebted to Hoyt for making this clear, even though unwittingly. Having men-

tioned the possibility that cultural change is independent of human voluntarism, I prefer to drop the matter since the literature on the subject is already long and somewhat tedious.

The recurrent assumption of cultural economizing fails to take account of developments in the theory of economic anthropology during the past decade. Largely inspired by Karl Polanyi, various social scientists, of whom the latest, but certainly not the last, is George Dalton (1961), have pointed out the dangers and errors that lie in the application of formal, classical economic theory to non-industrial, non-market economies. Most of these persons have laid particular stress on the inadequacy of the concept of scarcity as a guiding principle in comprehending value. If the scarcity-based model does not even fit all economies, much less can it fit all culture. Clearly the problem of choice among scarce means is not the key to the concept of cultural integration.

Thus Hoyt's paper leaves us much where we were in 1937, when S. F. Nadel wrote about Benedict's ideas (1937: 272):

The most important question which Dr. Benedict's theory leaves unanswered concerns the very nature of the *gestalt-like* patterns of culture. They are defined as "consistent patterns of thought and action" which mould and shape heterogeneous "items of behavior," "in obedience to (the dominant) purposes" characteristic of a culture. . . . But what are these purposes? And what is their objective? They seem to spring, fully grown, from history like Minerva from Jupiter's head.

Hoyt's main suggestions for going beyond this point are based on the assumption that culture is to be understood as human behavior, and that the locus of integration is in the individual mind. This is implicit in suggestions that "in major culture change, integration involves, or may involve, deep disturbances in the unconscious." The interest and significance of such a statement is not challenged, but I would note that the relevance of such disturbances to culture integration per se is dubious. The disturbances are possibly symptomatic results of change, but not causes of new integration modes. The theory is not improved by being supplemented with economic theories of standards of living or welfare.

The questions that must ultimately be asked in pursuing the problem of cultural integration are neither fuzzy nor vague. They will not be questions of ethics or of mystical essences. Instead, the questions will have to do with the selective forces that operate on cultural systems; and this, in turn, will require much more sharply defined

concepts of culture and its structure. There is a great deal of confusion in previous treatments of selective pressures in cultural dynamics, and one particular locus of error and obscurity may be mentioned. I refer to the frequent, if generally unstated, assumption that the selective pressures operating on a culture, and hence upon its integration, are basically those that have to do with the maintenance of the population of culture carriers, a confusion of levels of abstraction. Elsewhere in the realms of science there are suggestive models which may be of advantage here. The concept of selection is employed by physical chemists like Blum (1951: 162-63) and Calvin (1956: 253-54; 1959: 366), but they use it in ways that are clearly different from those approved by biologists. It suggests that in dealing with selective cultural pressures we need not depend on translations of biological models and this, in turn, helps to free us of misleading and futile attempts at reductionism.

Let us return at this juncture to an old set of concepts which are variously identified but known most widely as "core and superstructure." While the utility of this distinction seems nil in problems of physical-chemical integration and also in biological organic integration, it seems quite potent with regard to culture. Though given greatest currency by Marx, Engels, and their intellectual heirs, the concept appears, apparently independently, in the works of various social scientists, including several anthropologists. For the sake of example we may cite Leslie A. White, Julian H. Steward and Walter Goldschmidt. In White's view (1959: 19):

the fact that these four cultural categories [technology, sociology, ideology and attitudes] are interrelated, that each is related to the other three, does not mean that their respective roles in the cultural process are equal, for they are not. The technological factor is the basic one; all others are dependent upon it. Furthermore, the technological factor determines, in a general way at least, the form and content of the social, philosophic and sentimental sectors.

Julian Steward's views resemble White's on this point, though further reading reveals wide divergence. Indeed, Steward is not always consistent in his definition of core. Closest to White is this one (1955: 37):

cultural core—the constellation of features which are most closely related to subsistence activities and economic arrangements. The core includes such social, political and religious patterns as are empirically determined to be closely connected with these arrangements.

But Steward wavers in this definition, for in a later passage (1955: 89) he makes cultural core an heuristic device of the investigator, rather than a postu-

lation of universal characteristics of culture. Thus he moves finally to stating that (1955: 93):

Obviously, the diagnostic features of any given era—the *cultural core*—will depend in part upon particular research interest, upon what is considered important; and there is still a healthy if somewhat confusing disagreement regarding these matters.

Walter Goldschmidt echoes White and the first of Steward's positions given above. He says that "We view technology as the mainspring of the changes that have made for evolutionary progress" (Goldschmidt 1959: 114). Going further, Goldschmidt links this statement to the general problem of cultural integration (1959: 116):

The most important point about these consequences of technological development is that they in turn have their own consequences: individually and together they require adjustments in the social system, alterations in the manner in which the components of social organization can be or must be fulfilled.

From these views we see that one major approach to cultural integration, far from construing it as a matter of spiritual *Geist* or as a mystical property of nationhood, interprets it as the necessary and inexorable structural-functional consequence of the presence of certain fundamental complexes and institutions. This group of theories states also that changes in these fundamentals will cause changes in integration; though provision is made for some feedback from the superstructural aspects, these are seen as generally much weaker and incapable of effecting large-scale changes in the core or, for that matter, in the general pattern of the culture. In any case, these theories treat integration as a process rather than as a property of culture. Furthermore, with such a scheme it is possible to approach problems of society and culture with a wider range of potentially fruitful hypotheses. It is, for example, not necessary to homogenize the society for analytical purposes, since this is a scheme which permits analysis in the face of class, regional, and other sub-cultural variation. It facilitates the study of intergenerational change by avoiding very interesting but, for these purposes, irrelevant excursions into psychology. In broad terms, such a scheme is essentially dynamic, geared to the explanation of change and evolution, and it avoids the pitfalls of mere typology.

[*Ann Arbor, Michigan*, 21.3.61]

By STEPHEN FUCHS*

1. What the author has to say about the definition and types of integration is correct and well expressed. But she has completely ignored two factors which, to my mind, play a very impor-

tant part in the integration of culture: tradition and history. Her approach to the problem is psychological; but the approach must also be historical. For tradition and the past play an important part in the cultural integration of a people. This can be seen and noticed perhaps nowhere better than in India, where so many different peoples live side by side, still tenaciously clinging to their own distinctive cultures, mainly because of their different traditions, which they cherish and which allow them to adopt one cultural trait of an alien culture, and not another one.

It is correct to say, with Malinowski, that culture is a functional whole; but, as such, culture is as much a product of tradition and of the past history of the people that possess this particular culture. The integration of culture is not fully explicable without the inclusion of the factors of tradition and history.

2. The author nowhere mentions the theories of Schmidt, Koppers, and other German adherents of the Culture-Historical School. The scheme of the culture-circles may have been abandoned, but many ideas of the Culture-Historical School about the integration of culture seem to me still valid. They should not be completely ignored.

3. At the end of her paper, the author mentions welfare economics in passing. Now this concept is, in my opinion, of utmost practical importance in the present world-wide plans to help the so-called under-developed areas. With the best of intentions, but with an often complete disregard for the existing culture and cultural standards, the "privileged" nations often force their goods on peoples who are incapable of using them and of integrating them into their own culture. The result is a terrible waste of goods, great disappointment for the generous donors, and dissatisfaction and disillusion for the recipients.

[*Bombay*, 15.3.61]

By GEOFFREY E. GORER*

In Hoyt's very interesting review of the concepts of integration of culture, I miss any emphasis (it is perhaps too much to say any awareness) that societies are composed of human beings who have to learn their culture. Only in volition does Hoyt pay consistent attention to the human beings operating the culture: "All parts of a culture are dependent on a human choice. . . ."; and again: "Man can choose nothing that is not a part of culture or does not become so by choosing it."

In explaining a market economy, this emphasis on conscious choice is probably adequate; but for cultures as wholes the list of *motives* must surely be somewhat larger, and must take into ac-

count short-term and long-term, conscious and unconscious motives.

It seems to me that one approach to the concept of integration of culture could be through the consistent investigation, with all the elaboration of post-Pavlovian learning theory, of the learning which is necessary for individuals if they are to partake in a culture. Integration has to be intra-cerebral as well as intra-cultural; there are limits to the amount of contradictory motives which an individual can hold simultaneously without psychological distress. This leads us to expect *coherence*, though not complete consistency, in individual behaviour. If members of a society (or a subgroup within a complex society) undergo the same or similar learning, we should theoretically expect that their motives would be coherent, and their behaviour integrated with their motives; it is this coherence and the subsequent behaviour which is, I think, described as integration.

The quotations from Radcliffe-Brown (1933) cited by Hoyt imply a similar attitude to the concept of integration; but his psychology, founded on "a system of sentiments," now appears inadequate and essentially static.

If the concept of motives were thoroughly explored, it might be possible to get indices of cultural integration by tabulating (for example) the extent to which different cultures establish motives (*vdl.* wishes, drives) which cannot be gratified for the whole population; or demand contradictory behaviour ("Be peaceful and be ready to fight," "Be parsimonious and be generous," etc.) from the same persons in different contexts.

I am puzzled by one significance which Hoyt gives to the concept of culture (I imagine many commentators will have pointed out that she uses the term with a number of different significances). She writes:

Growth in culture is much less likely to occur through a policy of nothing in excess than through disequilibria seeking to offset one another.

This seems to imply that "growth" in culture is desirable; but I find myself quite unable to understand what this "growth" implies, unless it is a synonym for technological elaboration. The notions that cultures grow, that some grow faster than others, and that fast growth is desirable seem to me difficult to accept except within the value system of a society (such as the U.S.A.) which puts a positive value on change.

[Haywards Heath, England, 4.10.61]

By EIICHIRO ISHIDA☆

Hoyt's rather exhaustive review of concepts of integration of culture omits an outstanding problem which, in con-

nection with her conceptual analysis, still remains for further discussion by anthropologists.

Hoyt makes it clear that the two main concepts of integration—integration by dominance of leading idea, and integration by interrelationship of parts—are not antithetical, but were always combined by early sociologists. The aim of integration of culture, which I think in practice as well as in theory combines these two aspects of integration, seems to be presupposed as self-evident in her article. It has greatly interested me, however, to read in the last section that in so-called welfare economics the underlying objective is to show what choice and arrangement of parts will bring about maximum returns for a given *purpose*.

I sometimes wonder if all parts of an animal organism are integrated in the most perfect or the fittest possible way for their survival in the struggle for existence. If other, perhaps better ways of integration have also been possible, then out of these possibilities one has been chosen in history by a particular process of natural selection. The same question may be raised with regard to a culture or a civilization as a functional whole. In practice the logical exactitude might be of very limited use, but the objective and method of welfare economics should be taken into serious consideration if anthropology, in the present-day world crisis, has anything at all to contribute to mankind and its continued existence.

How in each particular case a culture has come to be integrated in a specific way is the problem of "history" or the idiographic concern of anthropology. To discover any generalizing principles, regularities, or laws common to all the individual processes is the aim of its nomothetic aspect or "science." Whether and how, on the basis of these studies, we could manage to control and direct the process of integration of our civilizations, or contemporary human culture as a whole, for the purpose of our very survival would be the most worthwhile question which applied anthropology can raise today.

In the history of economic science, Marx, more than a century ago, emphasized the functional and structural correlation and integration of component parts of a culture. Though Marx himself, as Hoyt remarks of economists in general, never used the term "culture," he vaguely anticipated by the term "society" the concept of culture in the modern anthropological sense. He regarded the evolution of society as a progressive process of integration and disintegration of culture, and advanced

the theory or, in my opinion, working hypothesis of historical materialism, by which he claimed to have clarified the universal law operating within that culture process. In this hypothesis, both diachronic and synchronic aspects of the process are conjoined into a unified system of cultural dynamics. I think that anthropologists, whether they accept the theory or not, have the same problem to solve. But we must also remember that it is because historical materialism provides a theoretical basis for a certain "choice" in politics that the doctrine has exerted a powerful, even authoritative, influence not only upon politics, but also upon the academic circles of certain countries in the world. The validity of historical materialism should all the more be discussed openly in the light of contemporary cultural anthropology.

[Tokyo, 10.3.61]

By FELIX M. KEESING☆

The author demonstrates both the worth and the perils of crossing disciplinary boundaries. The worth of an economist's-eye view of this phase of anthropological theory is obvious. Furthermore, the cultural interpretations of economic theory are illuminating. The perils are primarily those of omission. Hoyt has the older scholarly record well in hand, but her analysis and bibliography on living scholars and programs mostly lags behind the current front of work. Missing, too, are such theorists on aspects of integration as Redfield (characteristics of great systems: the great traditions, etc.); Steward (levels of sociocultural integration, etc.); Lévi-Strauss and others (structural systems); Oliver and others (interaction systems); Spiro (functional typology, etc.); Spindler (cultural transmission); the later Kroeber (total-culture patterns, etc.); the later Bateson (communication systems); and various linguists and linguistically oriented scholars (semantic systems)—and this list is not exhaustive. Perhaps even more conspicuously absent are the integrational facets of what theoretically-minded younger faculty members and graduate students are now likely to be talking about, perhaps to the puzzlement of the senior staff: system theory, linguistic and other taxonomic models, componential analysis, cognitive theory, and organized storage of cultural data in computers. Here, too, anthropologists and economists have much in common to talk about. As one in the senior staff category, my impression is that, just as an earlier generation of anthropologists put aside the grand generalization on evolutionary or other

processes until unit historical and geographical phenomena could be assembled, so too the interpretations of macro-cultural systems (national character, etc.) are due to be played down until analysis of micro-cultural systems is better mastered. [Stanford, 15.3.61]

By ELEANOR LEACOCK☆

I must confess that I find Hoyt's article on concepts of cultural integration somewhat arbitrary in its selection of social scientists whose ideas are touched on, and somewhat limited in the aspects of integration which are covered. Further, although Hoyt states that "integration," as compared with "pattern," implies process behind structure and an interest in explanation, she hardly begins to touch on the different assumptions about cultural process and cultural causation which relate to different concepts of integration. The most surprising omission I find to be the concepts of integration and causality central to historically-oriented evolutionary views of culture. I should like, therefore, to comment briefly on evolutionary concepts of cultural integration and historical process.

Hoyt deals primarily with functionalist approaches to integration which imply equilibration as the primary mechanism of change. Parallel theories in economics see the equilibrating mechanism in society as the striving for a balance within the framework of limited choices at all levels, as culture moves "from one imperfectly attained equilibrium toward another." Different parts of culture are unequally integrated, with some parts more intrinsic than others. Which parts are more and which less intrinsic has so far remained variable and undefined, although ideology—i.e., one or more basic values—is always seen as playing a central, or even a dominant role.

The problems that functionalist approaches to culture pose as soon as one adopts a historical perspective have been pointed out often enough. A universal "integrative" movement toward "equilibrium" almost by definition describes one aspect of social process, but it can never of itself handle the profound and apparently *ordered* changes that culture has undergone in the total sweep of human history. By contrast, evolutionary theory is based on the primacy of technology and of economic structure in determining the nature of cultural integration in a society at any given point in time, and in causing a change, to use Steward's terminology, from one "level of sociocultural integration" to another. Much of Steward's work has been built on the principle that "advanced" cultures are differently integrated than "simple" cultures

(Steward 1955: 47-52). When White (1959: 205) asks, "How are diverse and conflicting elements to be integrated into a stable and orderly system?" he asks it in an historical context, for he continues, "And how is the continuity of such a system to be achieved? Finally, how and why do some social systems, specifically those of human primates, evolve from small homogeneous structures to large, differentiated, and specialized structures?" In his answer, White deals more explicitly than other evolutionists with the role of ideology, for his immediate reply is in terms of customs, ethics, and institutional structures, while in his total system he places these "sociological" and "ideological" components of culture as ultimately determined by the technological. "The technological factor," he writes, "determines the form of social systems, and technology and society together determine the content and orientation of philosophy." He goes on, "This is not to say, of course, that social systems do not condition the operation of technologies, or that sociological and technological systems are not affected by philosophies. They do and are. But to condition is one thing; to determine quite another" (1949: 364-66).

One might argue that we are now dealing with integration in quite a different sense from the way functionalists have been using it, but the answer can only be: Yes, that is the point. What is important to us here is that the centrality of material culture to the organization—or "integration"—of its other aspects, while seldom so explicitly or strongly stated, is in fact deeply embedded in the historically oriented concept of culture which has traditionally characterized the major part of archaeological and ethnological studies both here and abroad. As we well know, Morgan initially constructed a hypothetical series of developmental stages in culture history, which were based on the thesis that "the great epochs of human progress have (probably) been identified, more or less directly, with the enlargement of the sources of subsistence (Morgan nd: 19). While specific errors in Morgan's scheme, and problems inherent in the use of "stages" (or as we would say today, "models") have been pointed out again and again, nonetheless the main outlines of his reconstruction have stood intact. Furthermore, when assessing Morgan's work, even Lowie, ardent anti-evolutionist that he was, writes (1937: 27):

Notwithstanding the qualifications cited, evolution is a positive fact in material culture. . . . To admit this, together with the possibility that material conditions may affect other phases of life, is to open the way for a fixed sequence of social and religious phenomena. . .

And Hoyt quotes Malinowski himself (though she demurs) that every part of culture "exists as a means to an end," and all "activities, attitudes and objects are organized around important and vital tasks into institutions."

This is not to say that one would get anything like formal agreement among so diverse a group of anthropologists on the centrality of technology and economic structure to cultural integration and culture change. It would be hard enough to get agreement even among admitted evolutionists, who would vary markedly in their delimitations of "material culture" (to use what I think is a neutral phrase), and in their assumptions about the precise nature of its causal relation to other aspects of culture. Such differences of opinion are certainly evident among the younger anthropologists whose work is focused directly on approaching a closer understanding of this relationship. However, my point is that an historically-based assumption that a constantly evolving material culture is the primary determinant of cultural integration and culture change constitutes an approach to integration that should hardly be ignored. [New York, 25.3.61]

By A. H. J. PRINS☆

I should like to make two or three remarks with regard to Hoyt's paper.

First, the concept of *Weltanschauung*, whatever its correct translation into English may or may not be, cannot be equated with "integration." At the very most, it foreshadows "integration" as a categorical concept in the science of culture. At its very most I say, because, on the whole, a typically German notion like *Weltanschauung* is not readily apprehended by pragmatic American thinkers. (Even if she or he does not consciously follow Henry James or John Dewey, the typical American, like the typical Englishman, is apt to be a pragmatist.) Therefore the influence of Nietzsche or of other 19th-century German philosophers passes into Anglo-Saxon minds through a screen that may sometimes even have the character of a barrier.

Secondly, although the concept manifestly passed this screen in the case of Hoyt, it should not have given rise to the wish to include it in this article. For (in the context of Hoyt's paper) Nietzsche etc. should not be treated differently from, say, any of Radin's "primitive philosophers." If *Weltanschauung* is perceived as "integration," it is "integration" perceived as such, because "it just happens to be there." Any thinking person in a given culture, unless it is decadent or deeply riven, is apt to discover this "genius" or whatever he wishes to call it—even in an

adjacent culture (Nietzsche and the Greeks).

I was greatly surprised to find no mention of S. F. Nadel, whose *Foundations of Social Anthropology* has been, in my opinion, one of the major events in post-war developments. I would even go so far as to say that it may well be counted as one of the 4 or 5 greatest contributions ever made to anthropology. Are people in the United States so little aware of what goes on (or went on) in contemporary British social anthropology?¹

Nadel uses the term integration in an incidental fashion—and quite rightly so, for the context is sufficiently clear—when talking, e.g. of “the optimum size for efficient integration (of groups)” (1953: 164). But this concept of social integration is made a formal one when it comes under further discussion (1953: 165, 182, 371). Yet this formal concept does not concern us here. What concerns us is his use of the concept of cultural integration.

This concept is introduced in two stages. First, there is what Nadel calls the “integration of purposes.” But this integration of purposes, together with a “pattern” of logical consistency and one of psycho-physical causality, reveals a unitary character. And here lies the second stage of using the concept in a cultural sense. Only this time the coincidence and interrelations of the 3 alignments, and hence the over-all pattern of any given culture, is called “super-integration” or, by Bateson, “configuration.” I am not, of course, writing a review of Nadel's book. Therefore let it suffice to say that according to Nadel (and, since I have worked on problems of maritime culture, I fully agree with him) this concept of integration may well be a “coherent totality, whose character lies in the very contradiction” (1953: 391). Now this statement is more crucial than the reader might think.² For it shows (although Nadel does not say as much), that the concept of (cultural) integration need not stop short of being either an arithmetic or a biological³

¹As a Netherlands scholar, even if brought up largely in the tradition of Firth, Richards, Nadel, Little, and others, I may be permitted to say this without undue risk of being thought ethnocentric.

²As a statement it differs from Hoyt's quoting Benedict that some cultures are extremely integrated, whereas others show lack of integration.

³This seems to be so even in cases where some authors are not aware of the arithmetic implications of their statements. These remarks also imply that I cannot agree very easily with Hoyt's introducing the image of two poles, each at either end of a scale. Poles imply opposition and similarity, whereas “biological” and “arithmetic” integration are entirely different

loan element (for, although Hoyt does not come to that conclusion, it seems an important one to make), but on the other hand, in the old word-form it takes on a new lease of life on a level of cultural analysis—or should I say synthesis?—transcending biology and differing from arithmetic.

One final remark: Although Hoyt's concern is with the past, it may be of interest to look into the future. The Groningen Department of Cultural Anthropology, in its 6 years' plan for “Fundamenteel Cultuur-onderzoek,” which is still in an incipient stage, intends to tackle this problem via two new methods, or rather with a method (the trigrammatic method of measuring culture) and a model, which has been baptized the culture trispheroid. Neither model nor method has been in use in field research so far; but preliminary testing of both is being carried out, on a small scale for the time being, until sufficient funds are forthcoming. [Groningen, 22.2.61]

By M. S. A. RAO*

This article has led to a meaningful classification of many diverse concepts about the integration of culture. Such a task involves placing each concept in one or another broad general category, as well as the risks attendant on such a placement. But I fail to understand how the point of view of Radcliffe-Brown can be included in a discussion of integration of culture. Radcliffe-Brown stressed social structure, sometimes to the exclusion of “culture.” The passages quoted from his writings amply illustrate the point that the focus of his attention was social structure or system, within which systems of sentiments, customs, and beliefs play a part. He further held, while criticising Malinowski's concept of culture contact, that it was not a situation of the interaction of two or more cultures, but of “interaction of individuals and groups within an established social structure which is itself in a process of change” (1952: 202). Therefore, it is misleading to include Radcliffe-Brown's point of view under a study of the review of concepts relating to “Integration of Culture.”

A number of valuable concepts of relevance to the subject of the present article have been developed by anthropologists and sociologists who study things. A statement on, say, cultures' being integrated “about” or “upon” such-and-such values or themes refers to the second, whereas a phrase formulating that cultures are integrated wholes of ordered complexes, or whatever you have, refers to the first. There is no combining of the two, as far as I can see at present.

culture on the basis of social structure. Let me point out two sets of concepts that have been developed in India. The concepts of “Spread” and “Sanskritization,” developed by M. N. Srinivas in his book *Religion and Society among the Coorgs of South India* (1952), have greatly contributed to our understanding of the different levels of integration of the local and the Sanskritic Hinduism. McKim Marriott, drawing on the concepts of Spread and Sanskritization and combining with them Redfield's concepts of “Great and Little Tradition,” formulated the concepts of “Universalization” and “Parochialization” (1955: 191–218).

Hoyt has omitted a body of concepts developed by Redfield and his collaborator, Milton B. Singer, such as “Folk-Urban Continuum,” “Great and Little Tradition,” and “Primary and Secondary Civilization,” which are of great relevance to the study of integration of culture. Other omissions are J. H. Steward's concept of “Sociocultural Integration” (1955: 43ff); R. Linton's “Universals” (1936: 271–87); and M. E. Opler's “Themes” (1945: 198–206).

Hoyt has done well in pointing out the limitations of the deductive approaches of integration by dominance. I should like to go further and add that they are not only futile, but also a source of the greatest danger to humanity in general and to the development of the social sciences in particular. They promote national and international prejudices and animosities.

As an economist of long standing in the field of consumption studies, and an economist who has the point of view of the anthropologist or sociologist, Hoyt has made a real contribution in her reflections on problems of economic development and culture change. She rightly points to the gross neglect of culture pattern by economists in dealing with problems of economic development in under-developed areas.

[Delhi, 28.2.61]

By SURAJIT SINHA*

Elizabeth Hoyt rightly points out that “integration” may be conceived simultaneously in terms of dominant value configuration and as interaction of parts. It is now generally accepted that all societies are in a perpetual state of partial imbalance, tending towards integration as well as, to a certain extent, to further disintegration. It is quite striking that although the concept of integration has been rather firmly established in the social sciences for the past 30 years or so, there has been hardly any conscious attempt to assess the degree of integration of a particu-

lar society at various points of time, or to compare the relative degrees of integration of a number of societies. One is not quite sure whether integration is to be assessed primarily in terms of the extent and intensity of interrelatedness of the parts of a social system, or on the strength of the most crucial structure of a society and its dominant sentiments or ideas. In any case, it is time to undertake a number of such case studies before we go in for further theoretical statements on the nature of social or cultural integration.

It appears to me that integration involves two qualitatively distinct mental attitudes: (a) repetition of the past through *passive* conformity, and (b) *active* defiance or deviation by the highly creative few who rouse the hidden aspirations of a people and often lead a society towards re-integration. The relatively passive and active dimensions of the mind may profitably be distinguished and followed up in the study of the dynamics of integration. The social processes related to the active aspect of the mind may be pursued by examining the relation between social integration and creativity in moral, intellectual, and aesthetic spheres. It may be discovered that a high degree of social integration may not have that much of positive bearing on creativity.

I am tempted to cite here an insightful overstatement by Sardar K. M. Panikkar (1955: 99):

The social institutions of Hinduism are in no way integrally connected with either the outer or inner forms of religion. The inner forms of religion, its ethic, its general philosophy, its doctrines of direct realisation through *Karma*, *jnana* and *bhakti*, these are in no way connected with the customs, social forms and personal law prevalent among the Hindus. No one would argue that if the joint family were to be abolished by law to-day, or caste ceased to operate as an institution, Hindu religious thought would be affected.

[Nagpur, India, 5.4.61]

By KENZŌ TSUKISHIMA☆

It seems to me that a culture always has a certain kind of integration if it truly is a culture at all, although Ruth Benedict said that "lack of integration seems to be as characteristic of certain cultures as extreme integration is of others" (1934: 206). Of course, in some cases it may be difficult to find an integration and to describe it. But integration must be thought of as an essential feature of a culture, according to the very concept of the latter. Wherever we see a culture distinctly, we shall find some integration within it, though its parts are not equally integrated. Whether it is possible to describe the integration is another question.

Hoyt's analysis of the concept of integration is very instructive, especially

when she approaches it from the viewpoint of economics. True, social systems of consumption and standard of living show a close interdependence of parts, just as does the fabric of language. Hoyt's description here is an emphatic demonstration of the truth of Benedict's words: "The importance of the study of the whole configuration as over against the continued analysis of its parts is stressed in field after field of modern science" (1934: 46).

Hoyt, distinguishing two different concepts of integration, says that these two concepts are at different poles. However, I rather doubt this. For integration by a single idea or value, or a group of ideas or values dominating a culture, is thought always to contain integration by interaction among traits within the culture. Dominance within a culture by an idea or value, or a group of related ideas or values, will not take place unless strong interrelationship of parts exists in the culture at the same time, although the reverse is not always true. So, I should like to suggest that the first concept and the second concept are not different and independent; but rather that the first is no more than a particular instance of the second, and that the second is the normal concept of integration of a culture.

[Tokyo, 10.3.61]

By SHUNPEI UHEYAMA☆

According to Hoyt, there are two types of methods for explaining the fact of "integration of culture": that is, "integration by dominance of a leading idea" on the one hand; and "integration by interrelationship of parts" on the other. This analysis may be very instructive from the viewpoint of the history of theories dealing with the integration of culture. But I would ask a more critical attitude of the author. In order to be more critical, the author in the first place should have given a precise definition of the concept "culture," and then made clear the relation between "culture" and "society," and between "culture" and "value." As to the author's own theory on the integration of culture, as expounded in this article, I am afraid I cannot agree with it. It seems to me that her method fails to give the necessary consideration to the factors of social change and natural environment. If she had given these factors adequate consideration, she would not have adopted the so-called "deductive" approach.

[Kyoto, 10.3.61]

By TEIGO YOSHIDA☆

In anthropology as well as in other social sciences, it seems necessary to carry out both empirical and theoretical studies. Empirical researches cannot

be fully conducted without concepts and theories; and theoretical studies cannot be made without empirical data through which hypotheses are examined, resulting in their denial or verification. In this connection, it is valuable to examine, review, refine, and propose certain concepts as analytical tools for describing and interpreting field material, since better "tools" can produce better results. Hoyt's thorough review of various concepts relating to "integration of culture" is thus a significant contribution.

Hoyt's paper is instructive in many ways. According to the writer, there are two different emphases on integration: the concept of integration by a dominant value or values, and the concept of integration as interaction of cultural traits. She states that "both forces can—and perhaps must—exist together." Probably, a culture integrated by dominant values tends to accompany integration as interaction of cultural elements. Whether or not there are correlations between those two forces or aspects, this distinction seems to be useful in dealing with cultural changes. In my view, integration by dominant values tends to persist longer than the integration of the traits. My field material on changing Japanese villages seems to show that the loyalty to a small village community "*buraku*" has been to a great extent maintained despite a certain disintegration of the existing sociocultural system.

After reading Hoyt's article, one question occurred to me: What kinds of hypotheses would the author propose with the use of the concept of integration? As Hoyt says that "the term 'integration' usually involves an interest in explanation as well," one might expect discussion of some hypotheses by which attempts are made to "explain" certain sociocultural processes. Based upon the empirical data, it seems possible to formulate a hypothesis that certain traits which are less integrated and relatively isolated tend to change more easily than those which are more highly integrated (Yoshida 1958). Kroeber also maintains that "what decides between continuance or change seems to be whether or not practice has become involved in an organized system of ideas and sentiments: how much it is interwoven with other items of culture into a larger pattern" (Kroeber 1948: 402). In the Japanese villages I have studied, while such a relatively isolated practice as individual and voluntary appearances at shrines to worship certain deities has disappeared in accordance with the decline in religious belief, village festivals that were more interrelated with other elements of culture had more continuity. Furthermore, it seems that

once changes occur in the cultural sphere consisting of tightly interwoven elements, greater cultural changes or disintegrations are likely to be generated, whereas changes of relatively isolated traits produce less effect in other parts of the culture. My field experience generally supports this hypothesis, and also shows that the effects of certain changes proceed at uneven rates in different cultural aspects. Despite the disorganization of the traditional exchange labor system in accordance with the increased wage labor in Japanese peasant societies, the mutual aid patterns on such occasions as building or thatching a house, and at the time of births, marriages, and deaths has not been much affected (Yoshida 1961: Ms).

I fully agree with Hoyt when she states that "as soon as one equilibrium is approached another is sought, and the culture is always moving from one imperfectly attained equilibrium toward another." The manner of this moving varies. Thus, it is regrettable that Hoyt failed to consider that, while conspicuous disintegration or disequilibrium took place in certain cultural changes, this process did not occur in other cases, and that the reasons responsible for this fact were not discussed. Some of the reasons for changes without marked disintegration were proposed by Chance (1960) in his studies on Eskimo cultural change. In accordance with our comparative analyses of Japanese peasant communities under different urban influences, a community "buraku" maintains in large measure its social and cultural integration despite the changes brought about by mechanization and other variables. One of the determining factors for the continuity of integration seems to be the community's continued reliance on agriculture centering around wet-rice cultivation, which imposes limitations on mechanization and requires a certain amount of co-operation and mutual aid on the buraku basis (Yoshida 1961: Ms).
[Fukuoka, Japan, 17.3.61]

Reply

By ELIZABETH E. HOYT

The great problem in writing this paper was to achieve, in respect to the significance of the concept of the integration of culture, clear development of thought. On this, 3 comments:

1. *Omission of definition of culture.* I purposely omitted such a definition. The emphasis was on integration, and the analysis would apply to a variety of definitions of culture. It would apply also to such a concept as Radcliffe-

Brown's, though he did not like the term culture. Definition of culture is important in other connections, but nothing is better calculated to deflect attention from a main line of thought than a definition of which the reader does not approve.

I did, however, unwittingly betray one aspect of my own definition when I said that all culture depended on human choice. (I did not say it depended solely on human choice; or on individual, free, or conscious choice; or that cultural change was necessarily introduced by human choice in the first instance.) At least one commentator (Fried) appeared to disagree, and I am able to grant that culture could be defined (though not from the root meaning of the word) as having segments or aspects outside the realm of human reaction. Even so, the analysis would hold, though with such a definition it might be necessary to include an occasional footnote or modifying clause.

2. *Other omissions.* Commentators have made various references to other definitions I have omitted and to persons whose names I have not given, or whose work they believe I have interpreted or discussed inadequately. Some of these persons I did consider, and rejected, because although this is a review article I conceived it as essentially a review of ideas rather than a review of the work of persons. When an idea, for the purposes of this paper, appeared to me to differ very little from one I had already presented I did not go further with it, even though it used new terms: this was in the interest of the clarity of the analysis of the whole. One critic pointed out that I had failed to do justice to the work of a certain scholar; as a matter of fact, I had sent my statement of my theory to this scholar and he himself had passed on it as adequate for the purposes of this paper.

Nevertheless I must have omitted some references that should have been included, and passed over some significant analyses, and I am grateful to my critics for giving me their comments on these. I shall consider them for their future value, but there is not now time for me to pass on them all.

The history of ideas was omitted purposely because, as I saw it, this history, though interesting, would detract from the unity of the paper, in which the emphasis is on analysis. The history of ideas of integration is important enough to be a subject in itself.

I am much interested in the suggestion (Keesing) of an analogy between the services of the collection of unit historical and geographical data in connection with the generalizations of

early anthropology, and the present situation, in which micro-cultural analysis may be of service in the ultimate interpretations of macro-cultural systems. I did, in fact, give considerable thought to the value of such micro-interpretations, though largely from a take-off in so-called welfare economics, which today consists in great part of such micro-analysis. Such efforts have so far brought me no light, except as the exactitude of the micro-approach acts as a check on loose thinking. I should like to see this subject discussed by someone who does see such light; the time is over-ripe for it. In this connection, Prins' reference to the Groningen work on the trigrammetric method of measuring culture should be noted.

Keesing well says that this paper demonstrates the perils of crossing interdisciplinary boundaries. In economics alone there is much disagreement as to how ideas should be stated and as to the relative importance of the work of different men, particularly of the younger men. How many more conflicting currents must be met if one also attempts to steer through the seas of other disciplines.

3. *Significance.* I am well pleased by the references of several commentators (Ishida, Sinha, Yoshida) to an ultimate question, on which I do not touch: What lies behind integration, and how can one judge the degree to which a culture is integrated? The question is obviously of supreme practical importance in the case of cultures torn by conflict; and less obviously, but perhaps just as truly, of importance to all. Before it can be answered we must be concerned with what integration is, and have some agreement on its meaning. We have a long way to go, but the first steps are taken: the chord vibrates.

References Cited

- ABERLE, DAVID F. 1950. Shared values in complex societies. *American Sociological Review* 15:495-502. [DFA☆]
 ——. 1960. "The influence of linguistics on early culture and personality theory," in *Essays in the science of culture in honor of Leslie A. White* (ed. GERTRUDE E. DOLE and ROBERT L. CARNEIRO), pages 1-29. New York: Crowell. [DFA☆]
 ——. 1961. "Navaho," in *Matrilineal kinship* (ed. DAVID M. SCHNEIDER and KATHLEEN GOUGH), chapter 2. Berkeley: University of California Press (in press). [DFA☆]
 ABERLE, DAVID F., ALBERT K. COHEN, ARTHUR K. DAVIS, MARION J. LEVY, JR., and FRANCIS X. SUTTON. 1950. The functional prerequisites of a society. *Ethics* 50:100-11. [DFA☆]
 ALBERT, ETHEL M. 1956. The classification

- of values. *American Anthropologist* 58: 221-48.
- ANDERSON, ROBERT. 1960. "Reduction of variants as a measure of cultural integration," in *Essays in the science of culture in honor of Leslie A. White* (ed. GERTRUDE E. DOLF and ROBERT L. CARNIRO), pages 50-61. New York: Crowell. [DFA☆]
- BATESON, GREGORY. 1936. *Naven*. Cambridge: Cambridge University Press.
- BENEDICT, RUTH. 1934. *Patterns of culture*. Boston: Houghton Mifflin.
- BIDNEY, DAVID. 1953. *Theoretical anthropology*. New York: Columbia University Press. [HB☆]
- BLUM, HAROLD. 1951. *Time's arrow and evolution*. Princeton: Princeton University Press. [MHF☆]
- BOAS, FRANZ. 1911. *Handbook of American Indian languages. Part I*. Bureau of American Ethnology Bulletin 40. [DFA☆]
- CALVIN, MELVIN. 1956. Chemical evolution and the evolution of life. *American Scientist* 44:248-63. [MHF☆]
- . 1959. Round trip from space. *Evolution* 13:362-77. [MHF☆]
- CHANCE, N. A. 1960. Culture change and integration: an Eskimo example. *American Anthropologist* 62:1028-44. [TY☆]
- DALTON, GEORGE. 1961. Economic theory and primitive society. *American Anthropologist* 63:1-25. [MHF☆]
- DAVIS, J. S. 1945. Standard and content of living. *American Economic Review* 35: 1-15.
- DU BOIS, CORA. 1955. The dominant value profile of American culture. *American Anthropologist* 57:1232-39. [HB, MSR☆]
- DURKHEIM, ÉMILE. 1947. *The division of labor in society*. Translated by George Simpson. Glencoe: The Free Press. [DFA☆]
- DYK, WALTER. 1938. *Son of Old Man Hat*. New York: Harcourt Brace. [DFA☆]
- EDGEWORTH, F. Y. 1925. *Papers*, Vol. I. London: Macmillan.
- FIRTH, R. 1939. *Primitive Polynesian economy*. London: G. Routledge & Sons.
- FREEDMAN, R. and others. 1956. *Principles of sociology*. New York: Holt.
- GOUDSCHMIDT, WALTER. 1959. *The ways of man*. New York: Henry Holt. [MHF☆]
- GREAVES, I. C. 1935. *Modern production among backward peoples*. London: G. Allen & Unwin.
- HALLOWELL, A. I. 1955. *Culture and experience*. Philadelphia: University of Pennsylvania Press.
- HERSKOVITS, MELVILLE J. 1949. *Man and his works*. New York: Alfred A. Knopf. [HB, MHF☆]
- HICKS, J. R. 1934. A reconsideration of the theory of value. *Economica* N.S.L:52-76.
- HOBSON, RICHARD. 1954. *Navaho acquisitive values*. Papers of the Peabody Museum of American Archaeology and Ethnology, Harvard University 42, no. 3. [DFA☆]
- HONIGMANN, JOHN J. 1959. *The world of man*. New York: Harper. [MHF☆]
- HOYT, ELIZABETH E. 1928. *The consumption of wealth*. New York: Macmillan.
- . 1938. *Consumption in our society*. New York: McGraw-Hill.
- . 1956. Impact of a money economy on consumption patterns. *Annals* 305:12-22.
- . 1960. Voluntary unemployment and unemployability in Jamaica with special reference to the standard of living. *British Journal of Sociology* 11:129-36.
- HUGHES, H. STUART. 1952. *Oswald Spengler, a critical estimate*. New York: Scribners.
- KARDINER, ABRAM. 1945. *The psychological frontiers of society*. New York: Columbia University Press. [DFA☆]
- KEESING, F. M. 1953. *Culture change*. Stanford: Stanford University Press.
- KLUCKHOHN, CLYDE. 1944. *Navaho witchcraft*. Papers of the Peabody Museum of American Archaeology and Ethnology, Harvard University 22, number 2. [DFA☆]
- . 1955. "Toward a comparison of value-emphases in different cultures," in *State of the Social Sciences* (Ed. L. D. WHITE), pages 116-32. Chicago: University of Chicago Press.
- KLUCKHOHN, FLORENCE R. 1950. Dominant and substitute profiles of cultural orientations. *Social Forces* 28:376-93. [HB☆]
- . 1953. "Dominant and variant value orientations," in *Personality in nature, society, and culture* (ed. CLYDE KLUCKHOHN, HENRY A. MURRAY, and DAVID M. SCHNEIDER), pages 342-57. New York: Alfred A. Knopf. [HB☆]
- KROEBER, ALFRED L. 1948. *Anthropology*. New York: Harcourt, Brace and Co. [HB, TY☆]
- . 1952. *The nature of culture*. Chicago: University of Chicago Press. [HB☆]
- KROEBER, A. L. and C. KLUCKHOHN. 1952. *Culture*. Cambridge: Harvard University Press.
- LINTON, RALPH. 1936. *The study of man*. New York: Appleton-Century. [HB, MSR☆]
- LOWIE, ROBERT H. 1937. *The history of ethnological thought*. New York: Farrar, Straus and Co. [EL☆]
- MACBEATH, A. 1952. *Experiments in living*. London: Macmillan.
- MAETTERLINCK, M. and others. 1926. *What is civilization?* New York: Duffield.
- MALINOWSKI, B. 1944. *A scientific theory of culture*. Chapel Hill: University of N. Carolina Press.
- MANNHEIM, K. 1952. "On the interpretation of Weltanschauung," in *Essays in the Sociology of Knowledge*, pp. 33-83. New York: Oxford University Press.
- MARRIOTT, MCKIM. 1955. "Little communities in an indigenous civilization," in *Village India* (ed. MCKIM MARRIOTT), pages 171-222. Chicago: University of Chicago Press. [MSR☆]
- MEAD, MARGARET. 1953. *Cultural patterns and technical change*. Paris: UNESCO.
- . 1959. *An anthropologist at work: writings of Ruth Benedict*. Boston: Houghton Mifflin.
- MOORE, G. E. 1903. *Principia ethica*. Cambridge: Cambridge University Press.
- MORGAN, LEWIS H. nd. *Ancient Society*. Chicago: Charles H. Kerr and Co. [EL☆]
- . 1881. *Houses and Houselife of the American aborigines*. Contributions to North American Ethnology 4. Washington, D.C.: Government Printing Office. [MHF☆]
- MURDOCK, GEORGE P. 1949. *Social structure*. New York: Macmillan. [HG☆]
- NADEL, S. F. 1937. The typological approach to culture. *Culture and personality* [Journal of Personality] 5:267-84. [MHF☆]
- . 1953. *The foundation of social anthropology*. London: Cohen and West. [HB, AJP☆]
- OPLER, MORRIS E. 1945. Themes as dynamic forces in culture. *American Journal of Sociology* 51:198-206. [HB, MSR☆]
- PANIKKAR, K. M. 1955. *Hindu society at cross roads*. Bombay: Asia Publishing House. [SS☆]
- PARETO, V. 1927. *Manuel*. Paris: M. Giard.
- PARSONS, TALCOTT. 1951. *The social system*. Glencoe: Free Press. [HB☆]
- PATTEN, SIMON. 1892. *The theory of dynamic economics*. Publications of the University of Pennsylvania, Political and Public Law Series 3, 2.
- PERRY, R. B. 1909. *The moral economy*. Boston: Houghton Mifflin.
- RADCLIFFE-BROWN, A. 1933. *The Andaman islanders*. Cambridge: Cambridge University Press.
- . 1952. *Structure and function in primitive society*. London: Cohen and West. [MSR☆]
- REDER, M. L. 1947. *Studies in the theory of welfare economics*. New York: Columbia University Press.
- REDFIELD, ROBERT. 1956. *Peasant society and culture*. Chicago: University of Chicago Press. [HB☆]
- ROBBINS, L. 1937. *An essay on the nature and significance of economic science*. London: Macmillan.
- SOROKIN, P. A. 1957. *Social and cultural dynamics*. Boston: P. Sargent.
- SOROKIN, P. A. and W. A. LUNDEN. 1959. *Power and morality*. Boston: P. Sargent.
- SPENGLER, O. 1926-28. *The decline of the West*. New York: Knopf.
- SPINDLER, G. D. and L. S. 1957. American Indian personality types and their socio-cultural roots. *Annals* 311:147-57.
- SRINIVAS, M. N. 1952. *Religion and society among the Coorgs of South India*. Oxford: Clarendon Press. [MSR☆]
- STEWART, JULIAN H. 1955. *Theory of culture change*. Urbana: University of Illinois Press. [HB, MHF, EL, MSR☆]
- TAX, SOL. 1958. "Changing Consumption in Indian Guatemala" in *Consumer Behavior* (Ed. L. CLARK), pp. 227-38. New York: Harper.
- WEISSKOPF-JOELSON, EDITH. 1953. Some suggestions concerning Weltanschauung and psychotherapy. *Journal of Abnormal and Social Psychology*, 48:601-4.
- WERTHEIMER, MAX. 1938. "Gestalt theory," in *A source book of Gestalt psychology*. (Ed. W. E. Ellis). New York: Harcourt Brace.
- WHITE, LESLIE A. 1949. *The science of culture*. New York: Farrar, Straus. [DFA, HB, EL☆]
- . 1959. *The evolution of culture*. New York: McGraw-Hill [DFA, MHF, EL☆]
- WILHELM, R. 1929. *The soul of China*. New York: Harcourt Brace.
- WISSELER, C. 1923. *Man and culture*. New York: Crowell.
- YOSHIDA, TETSUO. 1958. Bunka-henô no katei (Processes of acculturation: a trial formulation of hypotheses). *Shakai Jintuigaku* (Social Anthropologist) 3:1-16. [TY☆]
- . 1961. Cultural integration and change in Japanese villages (manuscript). [TY☆]
- ZIMMERN, A. 1922. *The Greek commonwealth*. New York: Oxford University Press.

More on Upper Palaeolithic Archaeology

SINCE PUBLICATION of Hallam L. Movius' "Radiocarbon Dates and Upper Palaeolithic Archaeology in Central and Western Europe" (CURRENT ANTHROPOLOGY 1:355-91), several persons have sent comments on it either to the author or to the Editor. The commentators include F. J. Brandtner, J. de Heinzelin, Hugo Gross, Janusz K. Kozłowski, Jiří Kukla and Bohuslav Klíma, H. Müller-Beck, Karl J. Narr, and Karel Valoch. Their comments are reproduced here, and are followed by a reply from Movius.

Comments

By F. J. BRANDTNER

I do not intend to review Movius' excellent paper, or comment on every detail that could be discussed. This would require almost a separate publication; and it would hardly add anything essential to our knowledge of geochronology, because we still lack some of the basic information needed to solve the problems in question. The gap in the evidence, however, cannot be closed by endless discussions, but only by the presentation of new facts based on careful investigations.

1. A discussion of radiocarbon dates without a clear stratigraphic framework (that is, without a detailed description of the site and the stratum from which a certain sample has been taken) is not useful and only causes confusion. Therefore, I agree completely with Movius in omitting the dates of the Karrestobel peat because of its uncertain stratigraphic position. However, I am sorry to say that the radiocarbon date of the compressed peat layer (lignite) from the Glütschtal outcrop is not much better established stratigraphically. As long as no detailed information is published, any correlation should be avoided. Such a date should not be used for making statements as fundamental as that concerning the termination of the Göttweig Interstadial.

2. Gross's conclusions concerning the duration of the Göttweig Interstadial, on which Movius relies, are not adequately supported. There is, for instance, not the slightest reason to correlate the Breda sample with the (supposed) end of the Göttweig horizon. There are also some reasonable doubts that the upper occupation layer of the Istállóskő Cave really belongs to the Göttweig Interstadial. The interpretation of the cave beds is based on astronomical theories and assumptions, rather than on geologic evidence. I believe that the portion of the cave beds that contained Vértes' "Aurignacian II" does not represent a weathering product

in situ, but rather a filling that had been washed in during a transitional climatic phase at the onset of the Middle Würm substage. The faunal and floral remains do not conflict with this point of view. There is not enough space here to discuss the Istállóskő publication in detail, but it must be emphasized that the evidence for the supposed interstadial age of the layers in question is, at least, very poor. In general, these layers seem to reflect conditions similar to those found in Willendorf, for example, and probably are also in the same stratigraphic position as the segment from that site that contained archaeological Layers 1-5. As Movius correctly notes, the close of the Göttweig Interstadial is probably much earlier than 29,000 to 30,000 years B.P., because fairly reliable radiocarbon dates obtained from evidently younger samples—from layers clearly above the interstadial soil horizon—accorded with 31,000 to 32,000 years B.P. in age.

3. To place the sample no. 2 of Oberfellabrunn (GRO-1901), taken from the loess a few inches above the uppermost chernosem horizon of the soil complex, and also the archaeological strata 1-4 of Willendorf II *within* the Göttweig Interstadial, as Gross did, is a manipulation that is unjustified. If we ignore clear stratigraphic evidence and simply arrange radiocarbon dates according to their figures or fit them to vague hypotheses and preconceptions, then we will hardly solve the problems.

4. The beginning of the Göttweig Interstadial also seems to be older than the supposed age of 42,000 B.P., because a date of 41,900 B.P. \pm 800 years was obtained from the first humus horizon above the reddish-brown weathering zone at Oberfellabrunn. This convincingly indicates that the change in climatic conditions which caused the decalcification must have taken place much earlier, probably somewhere around 48,000 B.P. or even slightly before. This estimate is based on reasonable extrapolation, and fits the most recent results obtained from study of deep-sea cores as well as from correlative continental deposits outside of Europe.

5. Concerning the radiocarbon dates

obtained from the Senftenberg locality, I must make a clear statement: I supervised the excavation of the archaeological layer (which typologically is closely related to the sites of Krems-Hundssteig and Getzersdorf) in 1952, and since that time kept the loess profile (exposed at a brickyard that was abandoned in 1957) under observation, registering all changes resulting from the proceeding work, and studying it intensively. The sketch of this profile given by Fink in his 1959 paper (in co-operation with Felgenhauer and De Vries) is incorrect, because a whole weathering horizon, partly still *in situ*, and a dislocated (parautochthonous) soil horizon, both approximately in the middle of the exposure, (that is, between the Krems soil and the Göttweig horizon), are entirely omitted. Also, a weakly-developed soil horizon (preserved only in remnants) below the Krems soil, subdividing the Riss loess into two distinct segments, is not shown in Fink's sketch. Fink probably drew the sketch from memory or fragmentary field notes, because the measurements are also incorrect. (Also, the locality lies at the Krems River, and not at the Kamp.) Therefore, the sequence used and briefly reported by Movius is incomplete.

Furthermore, Fink has completely misunderstood the position of the charcoal sample dated by the late Professor De Vries. It is correct that I took the sample in question with Fink on October 13, 1952, and marked it as "taken from the upper part" of the weathering horizon *presented*. However, this weathering horizon represents only the remnant of the former soil formation: the true upper part had been truncated by erosion, and further to the right this fossil soil, including the Ca-horizon, had been removed completely. The charcoal lens actually was only a few inches above the Ca-horizon (as some photographs I have at hand clearly demonstrate) within the horizon of decalcification (or loamification) close to the present surface, that is, to the surface which remained after the reduction by erosion. Later, the activity of rodents (marked by burrows) brought up some of the primarily embedded charcoal to the overlying (Middle Würm) loess, and this may also have led Fink to believe that the charcoal had been embedded at a time close to the end of the interstadial. But this does not prove to be correct. Actually, the charcoal was undoubtedly deposited during the loess accumulation preceding the loamification. Therefore, the charcoal cannot provide any date for the interstadial weathering process, but only for the prior deposition of the loess. A monograph, dealing in detail with the loess profile as well as with the archaeological

material, is in preparation and almost ready for print. It is hoped that in the meantime an additional charcoal sample from the same layer, secured in summer 1959, will yield a final reliable date.

6. Finally, some words about the radiocarbon dates obtained from two other localities, namely (a) the Salzofen Cave and (b) Aggsbach.

a) The few pieces of charcoal from the Salzofen Cave are undoubtedly highly contaminated (and there was so little material that a pretreatment was not possible). The conclusions of E. Schmid, based on her granulometric analysis of the cave sediments, are not diagnostic. Pedological and palynological investigations of the cave beds convincingly suggest an *interglacial* age (Riss-Würm), and this fits best with the paleontological results (which would not fit at all with the assumption of interstadial conditions, particularly not for this height in this region, as Ehrenberg also pointed out). The radiocarbon date therefore provides no support for Movius' assumption "that the Austrian Alps were also free of ice up to an altitude in excess of 2,000 meters during the Göttweig Interstadial." I recently finished a paper about this subject, summarizing all evidence at hand.

b) Further examination of the Aggsbach locality in the summer of 1959 revealed to me some new aspects. It is quite possible that the loess between the Göttweig soil horizon and the Paudorf soil had been almost completely removed by solifluction or erosion. Thus, the Paudorf soil may lie immediately on the remnants of the older soil complex, giving the illusion of a sequence like that at Willendorf. If this interpretation proves to be correct (several samples are not analyzed yet; however, I doubt that absolute proof can be obtained), then the archaeological layer on top would, of course, be much younger than previously supposed, and would then also be in accordance with the radiocarbon dates obtained. The figures are: sample a, GRO-1327: $25,600 \pm 100$ years; sample b, GRO-1354 (first run) $25,540 \pm 170$ years, and (second run) $25,700 \pm 280$ years B.P. (unpublished; Hl. De Vries by letter of Nov. 19, 1957).

The monograph of Aggsbach is already in an advanced stage.

[Washington, D.C., 4.1.61]

By HUGO GROSS

The comments inspired by Movius' (1960a) important publication on radiocarbon dates and Upper Palaeolithic archaeology in Central and Western Europe, which have been submitted by experts in Pleistocene geology and archaeology, respectively, reveal that we are still far from a general agreement on

the subdivision and chronology of the Last European Glaciation, notwithstanding the fact that this glaciation has impressed on the face of the continent very varied, most distinct, and most numerous traces. Furthermore, other publications demonstrate that the misunderstandings and confusion concerning the Würm sequence—misunderstandings which have now been accumulating for more than five decades—continue to increase.

This Würm muddle came about through the following factors:

1. Overestimating the geomorphological evidence.
2. Underestimating the palaeobiological (i.e., palaeofloristic and palaeofaunistic) evidence, insufficient indeed in the time prior to the introduction of modern pollen-analysis, the most objective method for distinguishing fossiliferous interstadial and interglacial deposits.
3. Uncritical reliance on obsolete statements and hypotheses advocated by prominent geologists.
4. Attempts by archaeologists to solve problems in Pleistocene geology, although they will not permit geologists to meddle in purely archaeological problems.

To investigate the sequence of the Last Glaciation, the geologist must, in the first instance, use these methods to study sections reflecting more or less completely the palaeoclimatic changes during the Late Pleistocene:

1. Granulometric, chemical, palaeobiological, and archaeological analyses of the sedimentary sequences in caves occupied by Palaeolithic man.
2. A very meticulous examination of the pedological, archaeological, and palaeobiological (particularly malacological) aspects of loess sections in the periglacial regions (above all, southeastern Central Europe), taking into account the possibility that they may be incomplete, as the result of erosion, disturbances due to solifluction, or downwashing, if the loess strata are on too steep a slope.
3. Stratigraphical study of the last drift.
4. O-18 and C-14 analyses of deep-sea cores from warm parts of the Atlantic.

Finally, the geologist must have as many strata as possible dated by C-14 measurement. This method has proved to be reliable for the last 20,000 years; but it is precarious for samples (at least for charcoal) between 20,000 and 70,000 years old, because of the possible infiltration of younger organic substances that cannot always be extracted by routine chemical pretreatment. For instance, one charcoal sample after such a pretreatment was stated to be ca. 48,000 C-14 years old, and later, after a very rigorous special pretreatment, turned out to be older than 54,000 years. Since almost all the samples older than 25,000 years, according to the first measurement, after a special pretreatment turned out to be many thousands of years older, geologists must grow sceptical of the possibility of establishing an absolute chronology through the C-14

method for the first half of the Last Glaciation. Apparently, not only the dates older than 53,000 years measured after isotopic enrichment (Haring *et al.* 1958), but also all C-14 dates older than 25,000 years, are now to be regarded as minimum dates.

Unfortunately, Penck and Soergel, the founders of modern Pleistocene geology, were hampered by the then-existing state of Quaternary botany, and made several serious mistakes. Thus they unconsciously initiated the Würm muddle—a statement, however, that by no means can impair their outstanding geological achievements.

Penck (1882) at first considered the huge masses of Alpine gravel ("lower glacial gravel"), up to ca. 50 meters in thickness and underlying the only then-known Würm ground moraine (which was seldom over 1 to 4 meters in thickness) to have been deposited upon the northern sloping piedmont region between the Aare and the Salzach areas during the period preceding the Würm Glaciation. Later (Penck and Brückner 1909), he considered this "lower glacial gravel" to be of interstadial age, because he found that it is underlain here and there by remnants of a ground moraine, which he mistook for Würmian moraines. Accordingly, he advocated a *tristadial* Würm Glaciation subdivided by two interstadials: the Laufen-Schwankung (1st) and the Achen-Schwankung (2nd). In 1922, Penck incorporated these interstadials, together with the lower Würm gravels as well, in the Riss/Würm Interglacial, thus advocating a uniform (*einheitliche*) Würm Glaciation. Consequently, he did not take into account that the accumulation of the huge ice masses of a Pleistocene glaciation must have required a very, very long advance phase. A reliable palaeoclimatological evaluation of the *Schieferkohle beds* (laminated peat) within and below the lower Würm gravel, decisive for Würm geology, was not possible until 1953, when Reich published her excellent pollen-analytical dissertation. Anyone referring to Penck has to take into account the drastic changes in his opinions.

Aware of the improbability of the existence of a rather complete section within the area of the last drift, Soergel (1919, 1939) based his subdivisions of the Last Glaciation on the stratigraphy of the Young Loess (Y.L.), which he regarded as reflecting the major climatic changes that occurred during the Last Glaciation. This Y.L. overlies the fossil Krems soil, generally recognized as interglacial, which Soergel, on pedological grounds, correctly ascribed to the Last Interglacial (Riss/Würm or Eem)—a dating which later was verified by malacological evidence (Ložek 1955).

Hence, according to Soergel, this Young Loess (much looser than the Ancient Loess below the Krems fossil soil) had been deposited during pleniglacial periods of the Würm and Weichsel Glaciations: Y.L. I (sometimes containing Mousterian assemblages) in Würm I, Y.L. II in W II, and Y.L. III in W III (Y.L. II and III sometimes containing Upper Palaeolithic assemblages). The periods of Y.L. deposition were interrupted by two main interstadial intervals: W I/II strong ("Great Recession Oscillation" = Penck's Laufen-Schwankung), represented by the Göttsweig fossil soil; and W II/III weak (Penck's Achen-Schwankung), correlated according to Soergel (1925, 1939), with the Paudorf fossil soil. Recently, a third (weak) fossil soil, W Ia/Ib, marked by a loam stratum within Y.L. I, has been found in several loess sections from Czechoslovakia to northern France (Valoch and Bordes 1957).

Unfortunately, Soergel (1919) wrongly correlated W I with the Riss and Saale moraines. For a long time preceding the issue of Soergel's 1919 book, the range of end-moraines marked by the Lüneburg Heath, the Fläming, and the Katzengebirge had been taken for the outermost border of the Weichsel Glaciation by the Geological Survey of Prussia. Later (until 1939), they were regarded by a few geologists as evidence for a separate (Warthe) glaciation. After 1927, Woldstedt emphasized again and again that these deposits had demonstrably been formed by the Warthe Stadial, the last stadial of the Saale (Riss) Glaciation (compare Woldstedt 1958b: 11, 21, 22, 24, 32, 119); but Soergel adhered to the obsolete conception. The degree of weathering registered by the Warthe drift is approximately intermediate between that of the Saale and Weichsel drifts. Within the Riss drift region, Soergel found such an equivalent of the Warthe drift in the vicinity of Saulgau (Swabia), but later he wrongly extended the correlation of his W I with the Swiss "Maximum Glaciation," which constitutes the whole of the Riss Glaciation. The thought that formations of his W I (if not eroded) must have been buried by deposits of W II did not occur to Soergel, who ignored the fact that a very long advance phase had taken place before W III. Hence, Penck (1938), who could find no glacio-morphologic formations of Soergel's W I, wrongly refused to accept his scheme. Soergel's equation of W I with Warthe proved to be almost ineradicable. W II was correlated by Soergel with the inner Main Würm moraines (Brandenburg and Frankfurt), and W III with the outermost Main Würm moraine (Pomeranian), but both correlations were later corrected by

C-14 dating. Soergel's second mistake (1925) was his detailed subdivision (*Vollgliederung*) of the Pleistocene in terms of alternating "Glaciations" and "Interglacials" only. His third error was the correlation of his W I, W II, and W III with the last three cold maxima, and of the interstadials Achen-Schwankung and Göttsweig (1925), and Paudorf and Göttsweig (1939), with the warm deflections registered by the solar radiation curve of Milankovitch, a correlation long since disproved by radiocarbon dating. From all these blunders there ensued in southern Germany the misuse of Soergel's terms W I-W III for formations of a very different nature and age, particularly for the main Young End-Moraines, the W I moraine of which was alleged to have been overridden by the W II ice.

Aware of these mistakes made by Penck and Soergel, Movius (1960a) and the present writer (1960a, b) have critically assessed all the relevant C-14 dates and stratigraphical findings available in May 1960.

Reliably fixed, and with sufficient exactness, by almost 100 C-14 dates is the time of the close of the Weichsel and Würm Glaciations, while the Late Glacial periods are directly controlled by geological and pollen-analytical studies, i.e., from Oldest Dryas times onward within the areas of both glaciations and within their periglacial ranges. The oldest of these Late Glacial dates (ca. 16,000 B.P.) has been measured for Meindorf on a sample of calcareous gyttja; possibly this is somewhat too old, because a wood sample H-136/116 from Poggenwisch (dated on a sample of calcareous gyttja W-93: 15,150 B.P. \pm 350 years) was 12,980 \pm 370 C-14 years old. The onset of the Late Glacial (when the ice-sheet began to retreat from the Pomeranian end-moraine to the Belt, and thereafter to the Langeland-Samland end-moraines, the latter probably a little older than the sites of Meindorf and Poggenwisch) has not yet been dated by C-14 measurement.

Reliably, but far less exactly, dated is the onset of the Weichsel Glaciation (at Brørup in Denmark, Loopstedt in Schleswig-Holstein, and Amersfoort in the Netherlands). This has been accomplished by pollen analysis and by radiocarbon measurements, the latter indicating an age of between 75,000 and 70,000 B.P., on the basis of the evidence that the first Weichselian Interstadial is ca. 64,000 C-14 years old at Amersfoort, and the first Würmian Interstadial at Grossweil in southern Bavaria is more than 59,000 years old—at least ca. 70,000 B.P. (Andersen *et al.* 1960). The palaeotemperature curve of Emiliani (1958)

indicates an age of ca. 72,000 B.P.

The initial period of the Last Glaciation was obviously very humid, as evidenced by solifluction indicating the first stadials and by the widespread development of swamps and formation of peat, e.g. of *Schieferkohle*, on the northern margin of the Alps and in their piedmont region, marking the two early interstadials (cf. Andersen *et al.* 1960; Reich 1953). The first Weichselian Stadial is the Danish Middle Bed of the Herning section in Jutland (Andersen 1957). The first short Weichselian Interstadial (with local subarctic forests) is the Rodebaek Interstadial in Denmark and the Amersfoort XII Interstadial in Holland (around 64,000 B.P.); the first Würmian Stadial is the *Schieferkohle* stratum of Pollen Zone 10 in southern Bavaria, and the first Würmian Interstadial is the *Schieferkohle* of Pollen Zone 11 at Grossweil (older than 59,000 B.P., according to De Vries, letter of 12.2.59). The second Weichselian and Würmian Stadial was apparently weak. The second Weichselian Interstadial was longer, and at its optimum, around 59,000 B.P., it produced a cool temperate forest (with *Picea* even in England); it came to an end between 59,000 and 50,000 B.P. (possibly ca. 56,000 B.P.). Deposits of this interstadial have been dated by C-14 measurement at Brørup in Denmark; at Loopstedt in northwestern Germany (ending ca. 50,000 B.P., according to De Vries [1958: 11; GRO-1365], which is somewhat too young); at Chelford in northeastern England (Simpson and West 1958; Shotton 1959); and at Amersfoort (Amersfoort XIV: older than 53,000 years B.P., according to De Vries [1958: 12]). The only second Würmian Interstadial thus far dated by C-14 measurement is the *Schieferkohle* stratum of Pollen Zone 13 at Grossweil (GRO-1477: older than 55,000 B.P.; Professor de Waard, by letter of 31.3.60, giving the definitive date). The existence of two early interstadials of the Last Glaciation has been ascertained by pollen-analytical studies in Denmark (Andersen *et al.* 1960), Holland (Zagwijn, in press), Bavaria (Reich 1953), and Poland (Rutkowski 1959 [preliminary report] and information by letter).

In the Agro Pontino (compare Blanc *et al.* 1957: 83-93), an oak-tree trunk lying in direct contact with the marine sand of Raised Beach F (5.10 meters above present sea-level, consequently the Monastirian II raised beach) yielded the preliminary Groningen C-14 date of 59,000 B.P. (Blanc 1960: 375).

This date indicates that in all probability Raised Beach F was formed dur-

ing the Brörup-Loopstedt Interstadial. Therefore, the lowering of the Last Interglacial sea-level, registered by the Monastirian I (+15–18 meter raised beaches), set in at the beginning of the Last Glaciation and continued down to the level of Monastirian II (thus far ascribed to this interglacial)—a lowering of ca. 8–10 meters, provided this change in level was due to glacio-eustatic and not to tectonic causes.

The C-14 date GRO-2083: 55,000 B.P. \pm 1000 years (revised) places the hand-axe Mousterian assemblage of Lebenstedt (Tode 1953) in the transition from the Brörup-Loopstedt Interstadial to the very cold third Weichselian Stadial, in agreement with the palaeobotanical evidence.

During the past years, the long time-span between ca. 55,000 B.P. and ca. 16,000 B.P. has been the battlefield for exasperated discussions between competent geologists and incompetent archaeologists, pedologists, and geographers. Unfortunately there are no C-14 dates for the main end-moraines of the Weichsel and Würm Glaciations; the maxima of both glaciations (Brandenburg in northern Germany, Killwangen-Schaffhausen-Kirchseeon in Switzerland and southern Germany) can only tentatively be dated by long-range correlation with the maximum of the Wisconsin Glaciation, an event which has been fixed by several C-14 measurements at ca. 20,000 B.P. In France, the intensely cold interval during which the Proto-Magdalenian occupation was effected at the Abri Pataud, Les Eyzies (Dordogne), where a series of C-14 measurements (GRO-2069, 2071, 2081, 2100 and 2115) all fall between 20,000 B.P. and 20,600 B.P. (Movius 1960a: 368), most probably correlates with the Brandenburg phase. The cessation of widespread, wholesale formation and deposition of loess has not been dated by C-14 measurement, and it may be tentatively correlated with the close of the Pomeranian phase (ca. 17,000 B.P. or somewhat later).

The palaeotemperature curve of Emiliani (1955: 569; 1958: 271), which does not show the two earliest interstadials, registers two Würm maxima at 65,000 B.P. and 20,000 B.P.; from ca. 60,000 B.P. until 30,000 B.P. there is registered a slight interstadial warming-up (called "Laufen" by Emiliani), which was interrupted around 40,000 B.P. by a very slight cold interval. The loess sections in the periglacial regions show the Paudorf Interstadial (W II/III) and the Göttweig Interstadial (W I/II) fossil soils; in the sequence of cave deposits there is a loam stratum (normally ca. 20 centimeters in thickness), which has been assigned to the W I/II Interstadial.

The Paudorf Interstadial was at first dated by C-14 measurement at Pavlov, Dolní Věstonice, and Geesthacht as ca. 26,600 B.P. \pm 300 years until ca. 24,800 B.P. \pm 150 years (De Vries 1958: 13, 14). Dr. Zagwijn, a palaeobotanist and collaborator of De Vries, informed me (by letter of 18.7.60) that De Vries had measured additional C-14 dates for the Paudorf Interstadial (unfortunately not yet published), which fall around 30,000 B.P. Possibly the greater magnitude of this new figure is in part due to the fact that the samples were collected in the cultural layers which are lying upon the fossil soil. But this soil was formed during the interstadial as the result of the weathering of the uppermost stratum of loess deposited during the preceding stadial (W II); consequently, during the entire interstadial the surface of the Paudorf fossil soil constituted a land surface upon which Palaeolithic man could camp. Samples taken from the fossil soil do not date the interstadial, but rather the final part of W II!

The fact that the Paudorf Interstadial is very rarely registered within the cave sediments is suggestive of a rather short and weak climatic amelioration (from ca. 27,000 until ca. 25,000 B.P.). Around 28,000 B.P., a very cold ("arctic") climatic interval is evidenced by the Arctic Plant Bed in the Lea Valley, North London; its sample Q-25 gave a C-14 date of 28,000 B.P. \pm 1500 years (Godwin and Willis 1960: 65).

During the period from ca. 31,000 until ca. 27,000 B.P., during the substage W II (between the chronological horizon subsequent to Willendorf II/4, and the beginning of the Paudorf Interstadial), Y.L. II was deposited. This cool period was interrupted by a short cool-temperature interval, as evidenced by the following strata (formerly incorporated with the Göttweig Interstadial Complex):

1. The Karrestobel peat to the north of Ravensburg in Württemberg (C-14 dates GRO-1260: 29,000 B.P. \pm 500 years and GRO-1277: 28,840 B.P. \pm 300 years), placed by Penck (1901/09:422) in his Laufen-Schwankung and later (wrongly) in the Riss/Würm Interglacial, and by De Vries (1958: 15) in the Paudorf Interstadial.
2. The interstadial peat of Breda P 25^a in Holland (GRO-2008: 29,930 B.P. \pm 300 years and GRO-2007: 31,750 B.P. \pm 400 years), suggestive of a cool-temperate forest climate, according to Andersen *et al.* (1960: 40).
3. The Glütschtal lignite of the "Spiezer Schwankung" of Dr. Beck, near Thun in Switzerland (B-20: 29,000 B.P. \pm 1500 years).
4. The Dutch peat strata of Emmeloord (GRO-390: 29,000 B.P. \pm 5000 years?) and of Wouw (GRO-931: 28,500 B.P. \pm 540 years); and
5. Perhaps of Breda (GRO-936: 32,000 B.P. \pm 900 years). However, according to Dutch pollen analysts, all of these latter

peats contain ca. 90% of non-arboreal pollen suggestive of a pleniglacial ("arctic") climate, whereas their covering of sand and/or loess is indicative of the transition from interstadial to stadial conditions.

6. The Aurignacian II sites in France, ca. 29,000 B.P. according to Movius (1960a); whereas the fauna associated with the Aurignacian I sites is indicative of a very cold climate (see below).

If the Paudorf Interstadial began ca. 31,000 B.P., as held by several authors, it would have been a continuation of what has been termed the "Göttweig Interstadial," and would exclude the existence of a pure Y.L. II. But the following data (compare Movius 1960a: 360, 361) suggest the final portion of an interstadial climatic amelioration: The upper cultural layer (Aurignacian II of Vértes = Olschewian of Bayer) in the Istállóskő Cave yielded the C-14 date GRO-1935: 30,710 B.C. \pm 600 years. This date (possibly a little too young) also fixes the approximate age of the Olschewian layer in the Potocka Cave, situated at an elevation of 1,700 meters above sea level. Because of this great altitude, Penck (who could not know the absolute age of its cultural layer) wrongly assigned it to the Riss/Würm Interglacial, whereas Soergel (1940), Lais (1941), and Zotz (1951) correctly ascribed it to the Göttweig Interstadial W I/II, because it contained two Upper Palaeolithic assemblages (Aurignacian I and II of Vértes) of different age.

Of about the same age (measured by radiocarbon dating), and therefore of the same interstadial climate (also evidenced by the mixed interstadial fauna associated) is the open-air site of Willendorf II/1-4 (Movius 1960a: 361, 362), which yielded what Bayer, fifty years ago, described as a "Middle" Aurignacian assemblage, and provided him with the evidence for defining his term "Aurignac-Schwankung." The excavation carried out by Felgenhauer (1959) in 1955 ascertained that the cultural horizons 1-5 lie within a "complex of solifluction earth" containing black-earth bands, displaced portions of a loam horizon, silt, and other slope-wash layers, which Brandtner (1959: 184) correlates with his Fellabrunn (= Stillfried A) complex of fossil soils. Considering the steep declivity and the then-existing climate (more humid, according to Brandtner, and by no means cold), the complex is apparently due, not to (sub-arctic) solifluction (Brandtner 1959: 193), but rather to slope-washing. The most important cultural stratum, (and the only one that is *in situ*, according to Brandtner (1956: 163; 1959: 187), is Willendorf II/4, which has been given the C-14 date GRO-1273: 31,840 B.P. \pm 250 years (Felgenhauer 1959: 153). The cultural strata of Willendorf II/

6-9 (the latter not far below the surface) are contained within pure loess (Y.L. II) several meters in thickness, and their fauna reflects a colder climate (Brandtner 1959: 192). The *Fliesserde-Paket* containing cultural strata 1-4, consequently, was obviously deposited in the final part (in the second half, according to Brandtner 1954: 79) of the interstadial registered by the Fella-brunn complex, and is not post-interstadial as Brandtner later held (1959: 187, 192, 196).

In the loess exposure of Achenheim near Strassburg (Woldstedt 1958: 222-24), several artifacts of "Middle" Aurignacian type (not a cultural layer, and not dated by C-14 measurement) have been found in pure loess (Y.L. II), but most probably they have been down-washed because of the declivity of the underlying older loess strata.

At the Salzofen Cave in the Austrian Alps (Movius 1960: 361), situated at an altitude of 2,008 meters above sea level, the main Cave Bear layer containing "Alpine Palaeolithic" and charcoal fragments belongs to the Göttweig Interstadial (W I/II), according to the granulometric analysis of the cave sediments; the C-14 date is GRO-761: 34,000 B.P. \pm 3000 years, which is rather inexact (the sample was too small). The fact of the occupation of this site by Palaeolithic man in itself proves a mild climate, often taken for an interglacial because of the great altitude; the occupation around 34,000 B.P. could have been possible only because the then-existing Alpine ice was restricted to the valleys, and the receding glaciers were covered by rubble and gravel.

Comparison with contemporaneous strata dated by C-14 measurement in the Netherlands (Andersen *et al.* 1960: 40) results in the following inferences: for the Breda peat: lower portion (GRO-936: 32,000 B.P. \pm 900 years), the pollen diagram indicates a "pleni-glacial age," i.e., an "arctic" tundra-climate; also for the highly cryoturbated loamy peat layer, Amersfoort XI (GRO-1276: 34,730 B.P. \pm 500 years), the pollen diagram points to a very cold climate.

From France we have the following C-14 dates for early Upper Palaeolithic horizons in caves and rock-shelters, together with the palaeoclimatic inferences (Movius 1960a: 366, 367):

1. Grotte du Renne, Arcy-sur-Cure (Yonne): Châtelperronian, GRO-1736 and GRO-1742: ca. 33,600 B.P. The climate was milder than it had been during the preceding Mousterian, as evidenced by the fauna and the nature of the sediments.
2. La Quina (Charente): Aurignacian I (with cleft-base bone points), GRO-1493 and GRO-1489: ca. 31,000 B.P. A sub-arctic climate prevailed, as shown by the fauna.

3. Grotte du Renne, Arcy-sur-Cure: Typical Aurignacian, GRO-1717: 30,370 B.P. \pm 300 years. The contemporary climate was mild ("Interstade d'Arcy"), as demonstrated by Mme. Leroi-Gourhan's pollen-analytical research on the cave sediments (compare Breda P 25*).

In the Dordogne and Charente regions, there are many Aurignacian I occupations in beds composed of limestone conglifractions accumulated during an intensive cold interval, according to Movius; the associated fauna is of a high arctic type. However, the deposits immediately overlying these Aurignacian I horizons were formed during a short interval of relatively mild humid conditions which favored the extension of forests (ca. 29,000 B.P.).

It is apparent that the palaeoclimatic evaluation of approximately contemporaneous strata covering the ca. 35,000 B.P.-ca. 29,000 B.P. range is sometimes conflicting, but we do not know the real dates implied by the C-14 dates. Peat strata containing ca. 90% non-arboreal pollen (and a high pollen frequency!) may nevertheless be of interstadial age, because the interval of slight climatic amelioration was too short to permit the immigration of trees. For the forests had been pushed far to the south during the preceding very rigorous pleniglacial climate of long duration, and the continental climate with several cold intervals would have prevented reforestation.

A very crucial point is the stratigraphical relationship between the earliest Aurignacian horizon (ca. 32,000 B.P.) in Central Europe and the Stillfried A Complex, particularly the Göttweig loam zone (*Verlehmungszone*). As this zone consists of the uppermost layer of the loess deposited during the preceding stadial (W I) and weathered during the subsequent interstadial, the loam zone, of course, cannot contain any early Aurignacian assemblages of interstadial age. At Willendorf II, Culture Layer 4 was lying upon a sandy slope-wash loess containing remnants of an interstadial loam and some bands of black earth correlated with the Fella-brunn (Stillfried A) Complex by Brandtner (1959: 184).

At Senftenberg, the distance is almost 2 meters (Felgenhauer *et al.* 1959: 59). According to Brandtner (1956: 162), elsewhere the Aurignacian horizon lies closely above the loam zone. Unfortunately, these sites showing an early Aurignacian horizon a little above the *in situ* loam zone have not yet been published, but they are numerous in Austria (Brandtner in letter of 18.1.61).

The C-14 measurements by De Vries (1959: 87, 88; Movius 1960a: 360) of

the fossil soils characteristic of the Stillfried A Complex and of the basal portion of Y.L. II (most probably corresponding to the third, uppermost, weak humus zone) at Oberfellabrunn have shown that the sequence overlying the loam zone was formed between ca. 42,000 and ca. 31,600 B.P. The latter figure approximately matches the date of Willendorf II/4, but it may be somewhat older, because the sample contained many rootlets, perhaps not completely removed during the careful pretreatment. Somewhat earlier than 41,900 B.P. began the retreat of the Irish Sea glacier in northwestern England (dated by the Upton Warren samples: GRO-595 and GRO-1245, which for many years have been ascribed by the collector [Prof. Shotton of Birmingham] to the incipient interstadial W I/II [De Vries 1958: 13]).

The second C-14 date which matches well the C-14 date for the lowermost humus stratum immediately above the Göttweig loam zone is GRO-2593: 45,300 B.P. \pm 1,000 years. This was measured late in 1959 by de Vries for a sample of swamp peat, only 5-20 centimeters in thickness, which overlies a fossiliferous bed of lake marl ca. 90 centimeters in thickness (the whole deposit is estimated at most to cover a span of 2,000-3,000 years). This layer was exposed within the lower Würm gravel in a deep quarry which had cut into a large drumlin near Hörmating (between Munich and Rosenheim): it is situated in the area of the Würmian Inn glacier (Ebers 1960; Gross 1960a, b). The peat contained some small pieces of *Picea* wood (GRO-2595: older than 53,000 years), certainly floated into the swamp by a meltwater stream from an eroded bed of *Schieferkohle* lying near the margin of the Alps at an elevation ca. 100 or more meters higher than Hörmating. Neither Ebers nor the present writer could find remnants of Old Moraine or Tertiary bedrock underlying the lake marl; only lower Würm gravel has been exposed by quarrying operations down to a depth of 15 meters. Consequently, this buried peat bog cannot have been formed during an Early Würmian interstadial, such as the one represented by the *Schieferkohle* beds at Grossweil (without a calcareous substratum!) within and below the basal portion of the lower Würm gravel, respectively. The peat (without any rootlets), overlain by up to ca. 4 meters of ground moraine and ca. 4 meters of sandy, completely decalcified, loess-like "lake clay," can scarcely have been contaminated by younger organic substances. Hence the peat, and not the wood embedded in it (which would

have soon disintegrated in the gravel), dates this buried interstadial bog. The molluscan fauna is indicative of a temperate climate, but cooler than the present (Ebers 1960) and certainly warmed by Foehn influence; moreover, the water basin was very shallow. Nearby, the lower Würm gravel, above the level of the interstadial layer, is covered by a weathered decalcified gravel stratum *ca.* 2 to 3 meters in thickness, which is overlain by Würm gravel that is moderately weathered at the top and covered by the only ground moraine, *ca.* 4 meters in thickness.

An analogous, and very probably contemporaneous, interstadial lake marl bed (unfortunately without a peat and weathered stratum cover), which yields an equivalent molluscan fauna, has been found by Dr. Ebers within the lower Würm gravel in the area of the Salzach glacier.

As evidenced by several deep gravel quarries, at some of which interstadial Early Würmian *Schieferkohle* beds are exposed near the base, there is one ground moraine only in the northern piedmont region. Accordingly, the Alpine glaciers did not protrude into the depressions of the northerly sloping piedmont region until some thousands of years subsequent to the Hörnating Interstadial. The ground moraine was deposited during the Main Würm substage (W III), and the over-ridden end-moraines are Main Würm and not Early Würm (W I) moraines! Prior to the glaciation, as earlier presumed by Penck (1882), the valley glaciers oscillating within the Alps poured out the lower Würm gravel, up to 50 meters in thickness, upon the depressions of the northern piedmont region.

The Götweig loam horizon of Oberfellabrunn did not contain humus substance in sufficient quantity for dating the interstadial loamification. At first, De Vries (1958) supposed that the Brørup-Loopstedt Interstadial produced this loamification, but later (De Vries 1959) he presumed that this weathering could have occurred during a separate warmer period around 48,000 years ago, rather than during the period between 67,000 B.P. and 57,000 B.P. (comprising the Amersfoort XII and the Brørup-Loopstedt Interstadials); but at that time an interstadial around 48,000 B.P. was unknown in Europe. Now it seems likely that the cool temperate humid climate of the Hörnating Interstadial (from *ca.* 48,000 B.P., or 47,000 B.P., until *ca.* 45,000 B.P.), shortly preceding the formation of the lowermost black-earth stratum of the Fellabrunn complex, is the time of the Götweig loamification at Oberfellabrunn. This correlation is in good

agreement with the former C-14 date for the Senftenberg charcoal (GRO-1217: 48,300 B.P. \pm 2,000 years) found by Brandtner, not below the base of the loam zone, but within the lowermost portion of this stratum, the greater part of the upper portion of the deposits at this spot having been denuded (Brandtner *in litt.*, 18.1.61). The Hörnating section within the area of the Würm Glaciation is apparently the equivalent of the periglacial Young Loess sequence from Y.L. I onward.

What is the palaeoclimatic evidence from Western Europe for the period between *ca.* 55,000 and *ca.* 35,000 B.P.?

In northwestern England, in the Avon Valley in Worcestershire, about 70 km from the front of the Irish Sea glacier, Shotton (1960) found a polygonal network of ice-wedges resembling the "patterned ground" in arctic Alaska; hence, a mean annual isotherm at least 13.5° C. lower than it is at present in Worcestershire. Shotton argues that this condition obtained at the time of the maximum of the Irish Sea Glaciation. This stadial correlates very well with the very cold substage subsequent to the Mousterian occupation of the Lebenstedt site (*ca.* 55,000 B.P.).

An equivalent of the Hörnating Interstadial in southern England is so far unknown; perhaps it was suppressed by the cooling effect of the nearby inland-ice. According to the Upton Warren and Fladbury C-14 dates (*ca.* 42,000 B.P. and 38,000 B.P., respectively), the Irish Sea glacier started waning at the time when the formation of the interstadial sequence of loess and humus zones overlying the Götweig loam zone began. During this period the climate in northwestern England must have been at most cool-temperate, because near Penkridge a large dead-ice mass, buried by morainic material and glacial outwash of the waning glacier, did not thaw until the Allerød Interstadial (Shotton and Strachan 1959).

In the Netherlands, according to Andersen *et al.* (1960), there have not yet been found any typical interstadial strata proving transitory afforestation between *ca.* 55,000 B.P. and *ca.* 35,000 B.P.; the radiocarbon and palaeobotanical data indicate a tundra-like, almost treeless vegetation, and consequently a more or less "arctic" climate. Two dates (GRO-1715 and GRO-1718: *ca.* 45,000 B.P. and 47,000 B.P., respectively) correspond chronologically with the Hörnating Interstadial; three others (GRO-1763, GRO-1852, and GRO-1779) cover the period between *ca.* 38,500 B.P. and *ca.* 37,400 B.P.

From France, there are unfortunately as yet no C-14 dates older than 34,000 B.P. (i.e., dates for Mousterian sites are lacking). Mme. Leroi-Gourhan (Com-

munication au Congrès de la Société Préhistorique Française à Monaco, Août 1959) has found a distinct interval of interstadial climate, from *ca.* 46,000 until *ca.* 41,000 B.P. ("interstade de Götweig?"), through the application of pollen analysis to the cave sediments of the Grotte du Renne at Arcy-sur-Cure.

The only date available from Spain is GRO-1556: *ca.* 47,000 B.P. for Layer G (Mousterian) at Gorham's Cave at Gibraltar, placed by de Vries (1959) in the "Early Würm II."

To summarize: the period hitherto defined as the "Götweig Interstadial," and dated between *ca.* 48,000 B.P. and *ca.* 30,000 B.P., was a span of time with an unstable climate, subarctic to cool-temperate, interrupted by more or less short cold intervals. However, doubt has been thrown on this conception by De Vries' dating of the Paudorf Interstadial as *ca.* 30,000 B.P. until *ca.* 27,000 B.P. (formerly *ca.* 27,000 B.P. until *ca.* 25,000 B.P.), and his redating of the Senftenberg charcoal (GRO-1771) as older than 54,000 years (formerly GRO-1217: 48,300 B.P. \pm 2,000 years).

The first statement means that the Paudorf Interstadial is the continuation of what has hitherto been called the "Götweig Interstadial" (thus eliminating W II and Y.L. II), and hence cannot be accepted (see above). The second statement means that the Götweig loam zone is far older than 48,000 B.P.—45,000 B.P. An alternative would be to establish an interpleniglacial interval with unstable climate between *ca.* 48,000 B.P. and *ca.* 25,000 B.P., as done by Woldstedt (1958b), who terms the period between *ca.* 50,000 B.P. and *ca.* 25,000 B.P. "Mittelwürm." The redating of the Senftenberg charcoal (provided that the new date is reliable) would most probably shift the Götweig loamification (i.e., the Hörnating Interstadial) into the cool-temperate Brørup-Loopstedt Interstadial, as advocated by de Vries (1958, 1959). But this interstadial came to an end *ca.* 24,000 years prior to the first appearance of Upper Palaeolithic culture in Europe, and hence Bayer's "Aurignac-Schwankung" would be either Woldstedt's "Mittelwürm" or the prolonged Paudorf Interstadial (*ca.* 31,000 B.P. until *ca.* 27,000 B.P.). If we should identify the Götweig loamification with the Brørup-Loopstedt Interstadial (*ca.* 61,000 until 56,000 B.P.), and date the Paudorf Interstadial as *ca.* 30,000 until 25,000 B.P., we should have to face the following implications:

1. Würm I would have lasted from *ca.* 72,000 until *ca.* 61,000 B.P., probably too short an advance period.
2. The loam stratum Y.L. Ia/Ib of several sections would correlate with the Amersfoort XII Interstadial (around 65,000

- b.p.); hence, the lowermost loess stratum Y.L. 1a would be the product of the first stadial of the Last Glaciation: a very unlikely correlation, because the first part of this glaciation was demonstrably very humid.
3. The formation and deposition of Y.L. II would have required more than 20,000 years: a scarcely acceptable inference, because Y.L. II is often no thicker than Y.L. I and is sometimes lacking because of erosion.
 4. The horizon of the (hitherto) earliest Aurignacian in Europe would not lie below the basal portion of Y.L. II, but (being ca. 24,000 years younger than this horizon) within its uppermost portion. However, the available C-14 dates measured on charcoal in France and Austria, perfectly consistent internally, cannot have been made too young by an equal contamination of the samples.
 5. The very old hand-axe Mousterian of Lebenstedt (ca. 55,000 B.P.) would post-date the Götweig loamification and fall into the interstadial period, marked by alternating humus and loess zones, overlying the Götweig loam zone; but such a Mousterian assemblage has hitherto never been found at this horizon in Central Europe.
 6. The identification of the Brørup-Loopstedt Interstadial with the Götweig loamification, moreover, disregards the C-14 dates for Upton Warren, Hörming, and the Châtelperronian and Olshewian cultural layers, and the distinct climatic amelioration these imply.

Consequently, the Götweig loamification in all probability is considerably younger than the Brørup-Loopstedt Interstadial, and correlates with the Hörming Interstadial, fixing a W I phase from ca. 72,000 until ca. 48,000 B.P.

The above argument cannot be refuted by the C-14 measurements applied to the Dolní Věstonice section late in 1959 by De Vries (unpublished; see Comment by Klíma and Kukla, p. 000), who found the entire Stillfried A Complex to be older than 52,000 years. But this date was measured on a sample from the basal portion of the (seemingly) Y.L. II, which consequently was a little above the Aurignacian horizon of Willendorf II/4, which is ca. 32,000 years old. The discrepancy in age (> 20,000 years) rules out the identification of the Götweig loamification with the Brørup-Loopstedt Interstadial as long as equivalent C-14 dates have not been measured for several other sections. The lower portion of the loess section at Dolní Věstonice, covering a rather steep slope, is likely to have been disturbed by dislocation, as evidenced by the three black-earth strata merging into one stratum at a higher level. Bohmers (1941b), Lais (1951, 1954), and Brandtner (1956: 146-50) have placed the sequence consisting of the loam zone and the overlying black-earth and loess packet in the Götweig Interstadial, whereas Knor *et al.* (1953), following Bayer (1927), ascribed it to the Riss/Würm Interglacial.

Within the above sequence, corre-

lated with the Stillfried A (Fellabrunn) Complex by Brandtner (1956: 146, 149), a second loam zone has been discovered (Ložek, by letter of 12.5.60). This second zone is alleged to substantiate the new hypothesis advanced by Ložek and Kukla (1959, 1960), who consider the Stillfried A Complex to contain the Riss/Würm Interglacial (represented by the basal loam zone of lessivé type) and the Götweig Interstadial (represented by the upper loam zone of non-lessivé type and the overlying packet of 2 humus and loess zones), because the Y.L. I is generally thin. Thus, they aim at a reconciliation of the short and the long Würm chronologies, established by Penck and Soergel, respectively. However, neither Brandtner nor Fink have ever found a vestige of a second loam zone within the Austrian Stillfried A Complex. This is likewise true of the pedologists who participated in the Austrian loess and terrace trip of the Deutsche Quartärvereinigung in 1955, when Brandtner and Fink demonstrated the Stillfried exposure, which occasioned considerable critical discussion. The possibility that this group could have overlooked such a second loam horizon at Stillfried and at many other sites is most unlikely. Consequently, the above hypothesis cannot be accepted.

In order to prove that the Last Glaciation was uniform (*einheitlich*; i.e., not subdivided by strong interstadials), as advocated by Penck since 1922, many scholars (especially pedologists, geographers and archaeologists in southern Germany and Austria), following the archaeologist Bayer (1927) and the geologist Göttinger (1935, 1936), ascribe the Götweig loam zone to the Riss/Würm, or Eem, Interglacial, and the Würm I to the Warthe and Young Riss stadials of the Saale (= Riss Glaciation). But there is not a single C-14 date proving that the Götweig loamification ceased between 75,000 B.P. and 70,000 B.P. Moreover, *nowhere* outside the Mediterranean region have strata been documented containing Upper Palaeolithic and "cold" Mousterian assemblages respectively, and separated by a layer of deposits classed as interglacial on the basis of fossil content (Brandtner 1956: 159). The Austrian Aurignacian did *not* appear at the beginning of the Last Glaciation (Würm), as claimed for example by Felgenhauer (1959), but ca. 40,000 years later! The pedological evaluation of the Götweig loam zone is still in dispute. Its loess structure, which is seldom preserved (according to Brandtner and Fink), is suggestive of mere decalcification below a steppe vegetation, or of weathering below a

steppe-forest cover consisting of trees characteristic of a continental climate—*Pinus silvestris*, *P. Cembra* (?), *Larix* and/or *Picea* (unfortunately, not anatomically distinguishable)—whereas the present steppe-forest flora of the same region consists of oaks (e.g. *Q. pubescens*, *Q. petraea*). The pollen grains of broadleaf warmth-loving trees, allegedly found in (lake?) loess and humus zones, appear dubious; furthermore, isolated charcoal fragments may have been displaced by burrowing animals.

A few scholars have mistaken the Götweig fossil soil (Fellabrunn or Stillfried A Complex) for an interglacial interval of the Last Glaciation, without proving that during this interval the inland ice of the first part of the Last Glaciation had waned to approximately its present limits. This glaciation began between 75,000 B.P. and 70,000 B.P. (Emiliani 1955, 1958; De Vries 1959); its duration was 60,000 or 65,000 years. To compress into this span of time two glaciations and one interglacial (Götweig) is absurd; but there are geologists who have actually believed in a Last Glaciation of 25,000 to 30,000 years' duration, and in an Eemian optimum at about 35,000 B.P., based on a bad section measured by the C-14 method!

Dating loess sequences by the underlying river terraces (particularly of minor rivers) is at best a very dangerous procedure if the age of the terrace is in dispute. Of course, the Low Terrace cannot underlie a loess packet up to 15-20 meters thick and containing two fossil soils, as maintained for Dolní Věstonice and Predmostí. In Moravia, the lower terraces are much older than the terraces of equal height on the Elbe and Moldau rivers (Prošek and Ložek 1957: 54). The reverse (dating a river terrace by the overlying loess sequence) is possibly more reliable, but only in arid regions where the Y.L. is subdivided by both fossil soils. On the distributive border of loess sections that are poorly subdivided due to too high a mean annual precipitation, e.g. in a small area of Swabia, a non-subdivided loess cover, allegedly representing the whole (uniform) Würm Glaciation in the opinion of Penck (1922), does not permit a correlation of this Young Riss Terrace with W I in order to "prove" that W I is Young Riss!

In concluding this critical review, one must admit that the C-14 chronology of the Last Glaciation, based on the original measurements of samples older than 30,000 B.P. and utilized by Movius (1960a) and the present writer, appears at the present time to be more plausible than does a chronology based on equating the Götweig loamification with

the Brørup-Loopstedt Interstadial—an equation that cannot be accepted until it has been confirmed by check-measurements at many additional loess sections. Unfortunately, the most crucial dates between ca. 70,000 B.P. and ca. 40,000 B.P., carefully measured after a routine pretreatment, have for the most part become many thousands of years older after a special strong pretreatment of the samples to remove any contamination by younger organic substances.¹ Therefore, the stratigraphy of the Y.L. is decisive, and should be accepted as the basis for a chronology of the Last Glaciation—as proposed by Soergel, but without the erroneous correlation with moraines and the solar radiation curve of Milankovitch. Substage W II, represented by Y.L. II within the Late Pleistocene loess section from the Willendorf II/4 cultural layer (ca. 32,000 B.P.) until the beginning of the Paudorf Interstadial (ca. 27,000 B.P.), was interrupted by a short cool-temperate interval around 29,000 B.P. (Aurignacian II in France, Glüttschtal *Schieferkohle*, Karrestobel peat). Consequently, the main substages of the Last Glaciation would be: W I from ca. 75,000 or 72,000 B.P. until ca. 48,000 or 47,000 B.P.; W I/II (Göttweig Interstadial Complex, a long period of unstable climate oscillating between “arctic” or “subarctic” and cool-temperate intervals of different duration) from ca. 48,000 or 47,000 B.P. until ca. 32,000 B.P.; Main Würm from ca. 31,000 B.P. until 10,000 B.P. (advance phase W II from ca. 31,000 B.P. until 27,000 B.P.; Paudorf Interstadial from ca. 27,000 B.P. until ca. 25,000 B.P.; Main Würm maximum from ca. 25,000 B.P. until ca. 16,000 B.P.; Late Glacial from ca. 16,000 B.P. until ca. 10,000 B.P.).

This chronology, based on the emended relative Y.L. chronology established by Soergel and on the C-14 dates available in May 1960, has been utilized by Movius (1960a) and the pres-

¹According to information recently received by Movius from de Waard, this special pretreatment, which De Vries applied late in 1959 to fossil humus, aimed at extracting the fulvic acids for C-14 measurement. These acids turned out to be considerably older than the rest of the sample (humic and other acids). De Vries, therefore, assumed that only the rest of the sample had been contaminated by more recent material, and that the fulvic acids, which can hardly have penetrated into the investigated sample from deeper layers, yielded the correct C-14 date. Before accepting such dates, the geologist desires (1) a scientific explanation of why the fulvic acids cannot be contaminated by younger material as are the other acids of the humus; and (2) a re-examination of this method by applying it to a humus sample, the age of which has been reliably ascertained by pollen analysis and routine C-14 measurement as hitherto carried out (e.g. Allerød gyttja or Allerød peat).

ent writer (1960a, b). But, although the relative Y.L. chronology established by Soergel has been applied to the Last Glaciation by Breuil and Koslowski (1931-34) and adopted (without emendation) by most of the European Pleistocene archaeologists, and (with emendation) by European Pleistocene geologists (see Brandtner 1954, 1959; Woldstedt 1958a, b), we have stirred up several hornets' nests, which, contrary to expectations, have contributed to further confusion. Obviously, this shameful Würm muddle will not be clarified until geographers, pedologists, and mere archaeologists decide to abstain from interfering in the Würm problem, which can only be solved by stratigraphical and palaeobiological research. [Bamberg, 26.12.60]

By J. DE HEINZELIN

I can judge from field experience only the stratigraphy of sediments in Belgium, the north of France, and the U.S.A. For the stratigraphy of the deposits in the Paris Basin and southwestern France, I base my opinion essentially on the work of F. Bordes, D. de Sonneville-Bordes, D. Peyrony, and on several classical sections which I have had an opportunity to examine. For the stratigraphy of formations in Central Europe, I refer to the existing bibliography, principally in *Eiszeitalter und Gegenwart*.

The division of the Young Loess in Western Europe into 3 stratigraphic units separated by interstadial soils, as formulated by Bordes (1954) for the Paris Basin and by Compton for the Somme Valley, may be taken as the point of departure. The Interstadial I/II loam, overlying Young Loess I, often seems relatively less developed than the Interstadial II/III loam overlying Young Loess II. This may be an apparent effect due to the fact that the erosion at the base of Y.L. II is generally much more marked than it is at the base of Y.L. III. In Belgium we believe we can see that the Interstadial I/II loam (Sol du Clypot, and not Sol de Rocour, which itself is a true interglacial *fendillé*) is more of a true zone of alteration than is the Interstadial II/III soil (Sol de Kesselt). Neither of them, however, has reached a stage of development approaching *terre-à-brique* or that of a “gray-brown podsollic soil.”

The only absolute date for these formations that has been published is for the sample of peat from Godarville (Hainaut, Belgium): W-173—older than 36,000 years (cf. Movius 1960a: 365). With this peat, which is “cold” and even periglacial, is associated *in situ* a very fine Mousterian of Acheulian tradition: type A, with many side-

scrapers and bifaces, and a Mammoth fauna.

At the time of excavation, it seemed to me that the peat and the industry were situated below a thick layer of Y.L. II; but it is not impossible that this interpretation must be revised, and that the fossiliferous Mousterian deposits actually belonged to a phase of Y.L. I. In that case, the position of this industry would be comparable with that of Leobenstedt (cf. Movius 1960a: 358; also De Vries 1958; Narr 1959; Tode 1953).

The locality of Goderville, almost homonymous with Godarville, but situated in France (28 km from Le Havre, Normandy), also deserves to be discussed: it is, in fact, practically the only site which has yielded the Lower Périgordian in a stratified context. All attempts to equate the loess deposits with those of the caves must hinge on this point, which has not been made entirely explicit (Bordes 1954, fig. 127). It is known that Bordes has assigned to Y.L. III the sediment which overlies this Périgordian I, which would place the transition from the Mousterian to the Upper Paleolithic very late in the loess sequence. But the position there of the so-called “*industrie mate*” of Goderville under a rather atypical *limon à doublets*, and there only, is not entirely convincing.

If it should turn out (there or elsewhere) that the Périgordian I actually appeared at the base of the Y.L. II, many contradictions would disappear, quite as much with respect to correlations with the cave deposits as with respect to correlations with Central Europe. There would take place at the same time the transition from the Mousterian to the Szeletian industrial facies of Blattspitzen (Vértes 1959).

Conversely, the position of the Upper Périgordian within the Y.L. III, and perhaps at its base, is clearer. In 1948, at Villejuif, F. Bordes and P. Fitte (Bordes and Fitte 1949) had at first assigned the Périgordian III to the base of Y.L. III, an idea that was revised when Bordes (1954) reassigned this industry to the *cailloutis* between Y.L. IIIa and IIIb.¹

Without doubt, it would be pertinent also to discuss the Achenheim sections, such as they are known from the publications of P. Wernert (1957). There Wernert has distinguished a complex of Young Loess separated into two divisions by a zone of loamification with considerable solifluction. The base of this complex (sandy loess and down-washed soil, Schumacher's *Kulturschicht* pro-parte) yielded artifacts of Mousterian affinity (rather laminar

¹Salomonson has recently studied the deposits at Belloy-sur-Somme, but I have no knowledge of his publication.

Levallois). The number of pieces is too small for precise diagnosis, but one could expect to find in this level the transition from the Mousterian to the Lower Périgordian. Immediately above, the first level of the true Young Loess yielded a typical Aurignacian (3 pieces which are sufficient to make the attribution) and one Châtelperronian or Abri-Audi Point.

The soliflucted zone of loamification yielded an assemblage with Proto-Solutrean retouch, disc cores, one shouldered point, and one broken tanged point; the overlying patches of loam which there are associated with this zone have yielded an assemblage with flat retouch, one Gravette Point, and one end-of-blade scraper. Therefore, this level marks the passage to the classic Upper Périgordian. It is followed by another true eolian deposit lacking in stone industries, except at the top where a varied assemblage with lamelles (Epipaleolithic?) has been collected.

Recently H. Müller-Beck (1959), in commenting on this section, has posed the question where the Göttweig Interstadial should be placed in this Young Loess complex (Gross 1958; Ložek and Kukla 1959; Prošek and Ložek 1958; Woldstedt 1958).² Wernert, in fact, places his upper Old Loess (with a weathering horizon), immediately below his Young Loess. The answer becomes self-evident if, by altering Wernert's nomenclature, one makes his upper Old Loess a lower Young Loess: it corresponds to Bordes' Y.L. I. At its base there was found a fine Mousterian industry with a large convergent, ogival side-scraper, a *couteau à dos*, and a *biface foliacé* or disc; in the body of the loess, a Levallois flake; and in the weathering soil, again, objects of Mousterian affinity. Thus the latter culture layer would date from the Göttweig Interstadial and not from the beginning of the Last Glaciation.

It should be noted here that the loam of the so-called upper Old Loess does not correspond to a *terre-à-brique*, whatever may be its thickness (up to 4 m.). "According to J. Franc de Ferrière, it represents a slightly podsollic and lessivé soil. . ." The ecological conditions seem to have been those of an open forest of "taïga bordering cold steppe" type (Wernert 1957: 34). This is properly the description of an interstadial and not of an interglacial.

According to this view, Wernert's Young Loess would correspond to Bordes' Y.L. II and III. The loam separating these latter, everywhere showing extensive solifluction and attenuation, corresponds to a last interstadial during the formation of the

Young Loess, occupying the same place in the stratigraphic sequence as do the soils of Paudorf and Vistorna (Romania) in Eastern Europe (Samson and Radulescu 1959).

These attempted correlations are certainly not clearly established; one should recall, by way of conflicting evidence, the latest paleoclimatic curve published for the Weichselian of Denmark and the Low Countries, in which the Göttweig Interstadial did not appear, although the Paudorf was clearly marked (Andersen *et al.* 1960).

The Solutrean and the Magdalenian were certainly subsequent to the formation of loess in Western Europe. This does not mean that there were no longer any cold-climate eolian accumulations, for there certainly were wind-blown sands derived from local materials, principally in the neighborhood of the rivers and the Tertiary outcrops.

In and above these wind-blown sands, the Bölling and Allerød (also called Usselo) Oscillations are registered (Hijzeler 1957), the latter often very developed and extensive. Here the late Upper Paleolithic industries terminate: the Federmesser, or the industries with Azilian points, and the latest Magdalenian (Schwabedissen 1957).

It is a significant fact that one has every reason to believe that the loamification of the loess (regardless of which Young Loess or outcrop) was already well marked considerably before the Allerød Oscillation (Centrum voor Bodemkartering, Gent: unpub.). This leads one to suppose the existence of a forest vegetation under the conditions of a relatively temperate climate before the beginning of Late Glacial time, that is to say, very probably during the Solutrean and Lower Magdalenian. The loamification process proceeded anew during the Holocene, especially during the Neolithic and the Atlantic.

The loess succession of the "Last Glaciation" in Belgium can be summarized as follows:

- Local wind-blown sands with the Bölling and Allerød Oscillations
- Loamification, *terre-à-brique* pro-parte
- Young Loess III with subdivisions
- Loam, frequently eroded, with solifluction ("Sol de Kesselt")
- Young Loess II with marked periglacial characteristics
- Extremely eroded base of Young Loess II
- Loam less developed than a *terre-à-brique* ("Sol du Clypot")
- Young Loess I often absent, not deposited, or eroded
- Basal complex of Young Loess I, with temporary soils and humic zones
- Products of the redeposition of old soils and loess (cf. "Fendillé remanié")

It is scarcely possible to cite the evidence of the cave deposits in Belgium, because their stratigraphy is too poorly known, almost all the excavators, for the most part early, having invariably mixed the horizons. If one tries to summarize the succession of cave deposits in France, one obtains the following picture:

Above the deposits of éboulis containing the Mousterian with its various facies is recorded a more temperate period with Périgordian I (de Sonneville-Bordes 1960). The Aurignacian I is glacial, whereas the Aurignacian II and part of the Upper Périgordian are relatively more temperate (cf. Interstade d'Arcy, A. Leroi-Gourhan, in Movius 1960a: 367; similar facts at Isturitz, A. Leroi-Gourhan 1959; Movius, *in litt.*, Jan. 16, 1961). The Upper Périgordian in part, but especially the Proto-Magdalenian and also the Aurignacian V (de Sonneville-Bordes and Bordes 1958; Bordes 1959; Movius, *in litt.*) are entirely glacial and thermoclastic. The Solutrean and the Lower Magdalenian are certainly milder and cover a period of about 4,000 years (18,000 to 14,000 B.C.). The Upper Magdalenian, especially Vb and VIa, is again glacial. The transition to the Azilian is petrographically and pedologically too poorly known to lend itself to correct, detailed interpretation, but there is every reason to believe that the Azilian is Post-Allerød, or at most, Allerød; here it will be recalled that it is the Azilian point, and not the Gravette point, which characterizes, in part, the northern Federmesser industries, which also date from the Allerød.

If one now wishes to give oneself over to the game of correlations (regardless of the fact that some of the evidence is not clearly established but nonetheless transitorily useful), one will start with the most simple hypothesis in equating the Göttweig with the interstadial or major oscillation of the Young Loess.

Interstadial I/II = Göttweig = Fellabrunn = Weathering horizon of the "upper Old Loess" at Achenheim = Sol du Clypot in Belgium = the transition from the Mousterian to the Lower Périgordian (with the reservation of the considerations discussed above).

Interstadial II/III = Paudorf = Vistorna = Arcy-Isturitz = Sol de Kesselt in Belgium = Middle Périgordian.

Hitherto we have not given the consideration it deserves to Interstadial III/IV of the Solutrean and of the Lower Magdalenian, so important from the point of view of sedimentation (a halt in loess deposition in Western Eu-

²The interpretation proposed below is in agreement with that of Bordes (1960a).

rope) and of pedogenesis (the first appearance of loamification of *terre-à-brique* type). If it is necessary to give this a name, one might take that of Laugerie-Haute, where the thick hearths of the Solutrean are well-marked within the deposits containing very few thermoclastic elements.

The pre-Allerød loamification shows itself in several sections of the sandy silt region of Belgium.

Taking into account the sedimentary and pedological importance of this interstadial, we have proposed with R. Tavernier (Tavernier and de Heinzelin 1957) the name "Epi-Pleistocene," for the period of time extending from the beginning of the Solutrean to the Allerød or the beginning of the Holocene (from ca. 18,000 to ca. 9,000 B.C., nearly the same length as the Holocene). This concept is of particular interest for the study of soils, since it indicates that the development of soils of the *terre-à-brique* type took place or could have taken place before the beginning of the Holocene.

If, perhaps too boldly, one looks for correlations in North America, one perceives that the "Classical Wisconsin" can correspond only to Stadials III and IV of our Last Glaciation (de Heinzelin 1958). The Tazewell loess corresponds to Y.L. III of Western Europe; the Michigan, to Interstadial III/IV; and the Cary Stadial to Stadial IV; the Two Creeks interval corresponds, as is well known, to the Allerød.

It is known that the existence of an Early Wisconsin, buried beneath the Classical Wisconsin, is in the process of being demonstrated in the United States. This is the Altonian Substage in the loess. The moraines of this advance have everywhere been covered by the maximum advances of younger moraines (de Heinzelin 1958; Leonard and Frye 1960).

A general comment about the terminology proposed by Movius is required. Such expressions as "Early and Main Würm Stage," "Early and Main phase of the Middle Würm," "Late Würm Stage, including the Fennoscandian stage," are ready sources of confusion. Movius gives definitions for "Oscillation" and "Interstadial," but not for "stage," "phase," or "time." "Stage" is also used in connection with prehistoric cultures. One does not understand whether "interval," "time-span," and "period of time" are synonymous.

If I stress this point of terminology, it is precisely because most of these terms have an internationally codified stratigraphic significance (cf. International Subcommission on Stratigraphic Terminology, Circular no. 9, 1960).

The terminology recently proposed by H. Müller-Beck (1959) is open to

similar criticism. It is hard to conceive that his divisions could serve as the basis of a convenient and fixed stratigraphic code.

I propose that one use, quite simply, in French as well as English, "Stade" and "Interstade" (rather than "Stage," which is "Étage" in French), for divisions of a Glaciation. Thus the Last European Glaciation is composed of four Stades, I, II, III, and IV; and three Interstades, I/II, II/III, and III/IV. If there is need for a less precise or "informal" term, one could settle for "épisode" or "intervalle" (without capitals).

Following the customary system of terminology for "rock-units" or "formations" in stratigraphic geology, it would be better to assign a type place name to the different well-characterized Stades and Interstades. This is already practically the case for the Interstades, but not for the Stades, with the exception of Stade III, which is itself the Brandenburg moraine. [*Gent*, 26.1.61]

By JANUSZ K. KOZŁOWSKI

There seems to be little doubt that the application of absolute chronology by means of radiocarbon dating has opened up new vistas for Paleolithic prehistory. At the same time, many important problems have emerged in an entirely new light. These developments are well set forth in Movius' admirable article, which attempts to provide a new synthesis, correlating absolute C-14 dates with problems of Upper Paleolithic stratigraphy in Central and Western Europe. I should like to offer a few additional observations from the perspective of East-Central and Eastern Europe.

1. Correlating stratigraphic data with the geochronological data obtained by means of the radiocarbon method threw new light on the development of the Aurignacian, Périgordian, Magdalenian, and Epipaleolithic industries. But information on the leaf-point industries is still very scant. Whereas in the context of geochronological data the Solutrean industry in Western Europe appears to have been a relatively short and rather late episode, leaf-point industries undoubtedly played a much more important role in Central and Eastern Europe. On the basis of the radiocarbon dates already known, these industries could be connected with the date for material from the Salzofen Cave in Austria, which Movius includes in the so-called Alpine Paleolithic. Although no typical Szeletian (leaf-point) forms were found in the Salzofen Cave, the not very numerous stone tools from that site show certain analogies with those of the Reppolust Cave (near Peggau, Austria),

which furnished the leading Szeletian forms (Mottl 1951: 47, 48, 55). It should be added that the side-scraper found in the Salzofen Cave showed close analogies with the Tata industry in Hungary (Prošek 1955). This side-scraper was regarded as Mousterian by L. Zotz, G. Freund, M. Mottl, K. Ehrenberg, and others; as in fact it is, in the typological sense. The Tata industry, which has been radiocarbon dated at ca. 50,000 years ago (Vértes 1957), includes forms transitional between the Mousterian and Szeletian industries (Ehrenberg 1953: 27-28). Hence, the date of 34,000 years for the Salzofen Cave materials can be regarded as corresponding in time to the Szeletian industry, in which side-scrapers of similar type were present. These dates provide further proof of the antiquity of leaf-point industries in Central Europe.

A similar situation existed in Eastern Europe. The leaf-points from Hsakaia on the Kuban, representing the early Upper Paleolithic in its transitional phase between a local Mousterian industry and the undoubtedly Upper Paleolithic industry of the type of the 5th layer at Kostienki on the Don (with triangular leaf-points; Rogatchev 1957: 40; Gorodcov 1940), have been given, by an analysis of their radium and actinium content, an absolute date of 39,000 years. The date for the same materials, calculated by Jakovlev (1955: 380) on the basis of radium and uranium content, was 27,000 years.

The problem of the Szeletian industry is also related to a point raised in the comment by Vértes: the accretion of Mousterian elements in some groups associated with the second phase of the Aurignacian industry in Central Europe. Their presence could be explained by contacts with the Szeletian area, whose influence was manifested in the occurrence of Szeletian leaf-points at certain Aurignacian stations (Istállóskő, Barca, and others). It does not seem, however, that this could be regarded as a proof of an independent origin of "Aurignacian II" in Central Europe, corresponding to Bayer's "Olsheva industry." The primitive character of the Olsheva stone inventories could to a large extent be accounted for by the type of raw material, since the tools were made of rock, the working of which is always more difficult than flint, and tending to yield less distinct forms.

2. The problem of "Périgordian II," as it appears in the light of present research, should rather be regarded as part of the differentiation of the facial Aurignacian industry (*sensu stricto*). I am inclined to share the view of D. de Sonneville-Bordes that this was not the basic industry from which the Aurig-

nacian and Périgordian subsequently developed (as maintained by A. C. Blanc). Nor can the "lamelles Dufour" be interpreted as a chronological index for the Aurignacian group (*sensu stricto*). For "lamelles Dufour" occur as an important component of certain undoubtedly Aurignacian Central European groups, including the Hundsteig industry (Austria) mentioned by A. C. Blanc, and were also found at the Góra Puławska (district of Koźienice) in Poland (Krukowski 1939: 70-71), and at Tincova in western Rumania (Nicolaescu-Plopsor and Stratan 1960). These stations, in which "lamelles Dufour" occur in large proportions (larger than in Western Europe; amounting to 34.1% in Tincova, and perhaps even more in Góra Puławska), have been found to be not chronologically uniform, but represent different developmental phases of the Aurignacian group, from the beginning of Würm II (Góra Puławska) to the end of this stadial, or even the beginning of the interstadial Würm II/III (Tincova).

In Eastern Europe, "lamelles Dufour" are also represented in the typical Middle Aurignacian, and in very considerable numbers. At the rock shelter Siuren I in Crimea, they amounted to 41.06% of the tools in the lower layer and 15.5% of those in the upper stratum (Vekilova 1957). It should be added that the upper layer of Siuren I has been dated by means of the radium and actinium method at about 20,000 years B.P. (Jakovlev 1955: 380).

3. It is interesting to compare the more important radiocarbon dates for the early phase of the Gravettian (Upper Périgordian) industry. Whereas in Western Europe the Périgordian industry (IV) has been dated at 21,650 B.C. (Abri Pataud), in Eastern Europe the local industry of the similar phase has been fixed at 23,650 B.C. (Dolní Věstonice), while the dates for a corresponding phase of the Eastern Gravettian industry from the upper stratum of Kostienki I on the Don have been given as 23,000 years (radium and actinium content) and 28,000 years (radium and uranium content) (Jakovlev 1955: 380). These dates, and also a new explanation of the Périgordian II and III problem, combined with a typological analysis, raise serious doubts about the correctness of Peyrony's scheme, according to which the Upper Périgordian industry of Western Europe derives from the Châtelperronian industry.

4. Movius' synchronization of particular areas of the Last Glacial period with geochronological determinations is also apt to raise certain doubts. This is, however, a question to be settled primarily by the student of geology. Here I shall mention only the fact that

the Périgordian "VI," dated by Movius at the maximum of the Brandenburg phase, is recognized in Central Europe as the industry that extended farther north than any other Upper Paleolithic industries (not including the Epipaleolithic). A station with typical Périgordian VI forms (*lames à tronçatures retouchées obliques et esoides*) (Krukowski 1939: 75-76) has been found at Mały Antoniów, in the district of Starachowice, about 130 km south of the local counterparts of the Brandenburgian terminal moraines.

5. With reference to the Azilian industry, it should be observed that the C-14 dates for Central European sites fall very close to the Allerød Interstadial, which agrees with the paleobotanical data. The level of the dune station in Witów (district of Łeczyca, Poland), which contains short end-scrapers of the Azilian type, has been dated at 8885 B.C. (Chmielewska and Chmielewski 1960: 133). The industry at the Szekszard station in Hungary, situated on the lower terrace of the Danube, has been dated at 8540 B.C. This Szekszard industry, containing characteristic Azilian elements, is related to the emerging central Danubian center of the Azilian (cf. the new Rumanian materials from Baile Herculane). This center, like that of the North European depression, is, in the light of geochronological dates, younger than that of Western Europe. [Cracow, 13.2.61]

By JIŘÍ KUKLA and BOHUSLAV KLÍMA

These further comments on the review article by Movius incorporate new data in the form of results of recent radiocarbon analyses of samples from the well-known and frequently described section at Dolní Věstonice (Unterwisternitz) brickyard. Following exactly the detailed instructions of the late Professor De Vries, Klíma took additional soil samples for radiocarbon measurement and sent them to Groningen. The results, together with the description of the Dolní Věstonice brickyard profile, are given in Figure 1. The dates should be taken as minimum values only. De Vries asked for more material to obtain more accurate figures, and new samples were sent to Groningen; but since his death the results of these are still unknown in Czechoslovakia. According to the usual practice in C-14 analyses, the preliminary dates can be younger than the actual figure, but hardly older. The following conclusions may now be drawn from the new dates together with the other C-14 results published in Movius' article:

The Göttweig Interval

1. The well-known complex with chernosem soils at Dolní Věstonice is older than 52,000 years. No Pleistocene soil exists at Dolní Věstonice brickyard with marked humus accumulation which is younger.

2. Furthermore, no other Pleistocene soil in the area of Moravia, Bohemia, and Slovakia, which is markedly darker or higher in humus content, is younger than the Dolní Věstonice chernosem, with the exception of certain unusually well-preserved soils, undoubtedly of Paudorf age. This statement is based on previous studies of a large number of important loess sections in Czechoslovakia, which were examined in detail from both sedimentary and pedological points of view.

3. The chernosem soils at Dolní Věstonice have been described and stratigraphically compared with the Stillfried A Complex, Fellabrunn, and Göttweig soils of Austria. Although it hardly seems possible that such similar soil complexes in Moravia and Austria could differ in age, the radiocarbon-dated section from Fellabrunn contradicts this.

4. As for the Fellabrunn dates, the following letter from De Vries, written after he had completed the group of Dolní Věstonice samples, and omitting only some personal remarks, indicates their unreliability:

The samples arrived in good condition on December 4. No. 5 is being processed. I discovered that the roots are quite unequally distributed. I had altogether 10 X 2.3 kg, and have for the time being put aside 2 X 2.3 kg, because there were too many roots in it. Two further 2.3 kg. portions were not very suitable and I shall fortunately not need the humus from them.

I have measured FELLABRUNN 3 once again. This gave 37,000 the first time. During the summer I extracted new humus and got about 42,000. This was curious. Now, the humus-extraction had been performed in a slightly different way. (Higher concentration of alkali, as also with the UNTERWISTERNITZ samples). Thereby less loess went into colloidal suspension. This loess binds fulvic acids when acid is added in order to precipitate humus. In sum: for the first measurement, fulvic acids had been mixed in the humus, but not later on. I have repeated the extraction and separately measured the fulvic acids. The age of the fulvic acids is 32,000, definitely younger than the humus. Fulvic acids are partial decomposition products of recent roots, whereby the younger age is explained. An age of 42,000 for the humus is surely also too young. Perhaps part washed in from above. I shall now measure the fulvic acids for UNTERWISTERNITZ.

Little comment on the above letter is necessary. Contamination by more recent organic material, as described by De Vries, can be sufficient to affect the

result decisively. The more closely the analyzed soil sample underlies the younger vegetation zone, the less reliable is the date obtained. Taking more samples from complicated soil complexes is practically useless, since in such cases only the top soil can yield reliable dates. At Dolní Věstonice, recent roots have often been found at a depth of 15 meters (ca. 50 feet) below the surface. More or less similar contamination must also be expected in other loess sections.

5. If we take the Götting Intersta-

dial as the period when black humus soils of the Götting profile were formed, then it is older than 52,000 years. It is probable that the Götting Interstadial also includes Amerfoort XII, Brørup, Loopstedt, and the Musolini Canal log.

The Paudorf Interstadial

1. Many radiocarbon dates from the period between 22,000 and 31,000 years ago show evidence of a comparatively mild climate. The geological position of the two samples from the Dolní Věsto-

nice locality, which gave radiocarbon dates of 28,100 and 28,900 years B.P., respectively, is important in this connection. Both samples lay in the upper part of a strongly decalcified soil with a marked Ca horizon at the bottom. Close above this soil, but not within it, lies the main Gravettian horizon of the mammoth-hunter's settlement, which is 24,000 to 26,000 years old near Pavlov. A very similar time position is shown by the so-called Farndale Substage of the Wisconsinian in the U.S.A., which is limited by the date $31,800 \pm 1200$

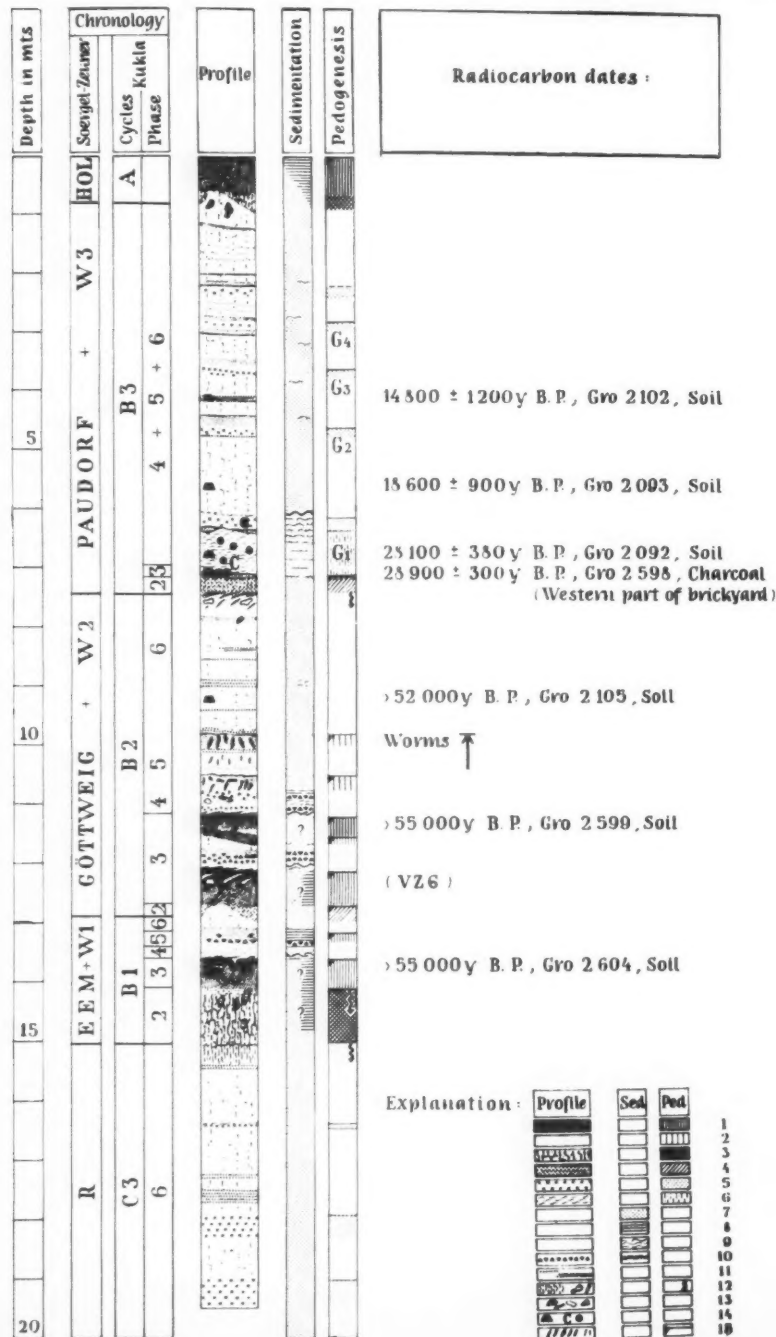


FIG. 1. PROFILE OF DOLNÍ VĚSTONICE LOESS SECTION WHERE SAMPLES FOR RADIOCARBON MEASUREMENT WERE COLLECTED

The data presented in older published descriptions were also observed here. The profile is located in the center of the eastern wall of the abandoned brickyard. At the edges the layers are marked: they are well developed only in the section above the abandoned wine cellar at a distance of about 35 meters from the main profile.

1. Chernosems and other soils rich in humus.
2. Feeble humus soils of the pararendsina development sequence.
3. Well developed lessivé.
4. Incompletely developed soils of the lessivé sequence.
5. Initial forms of pseudogleys.
6. Decalcified loams.
7. Deposited by wind.
8. Deposited by water.
9. Deposited or strongly disturbed by solifluction.
10. Pellet sands (= sediment built fully or partly of grains of loam, that were redeposited by quick strong water transport during torrential rains, following a period of drought. In such cases the dried loam fragments were not dismissed during the short time of redeposition).
11. Quartz sand interlayers.
12. Secondarily precipitated carbonate in the form of concretions or dispersed impregnations.
13. Krotowinas.
14. Samples for radiocarbon dating and charcoal fragments (C).
15. Worm channels; surfaces of individual soils.

years B.P. (W-638), obtained from the Wisconsin till (Frye and William 1960). For the most part, data which we usually associate with the Paudorf Interstadial are connected with Gravettian sites. But no one can say whether or not the period of these Gravettian occupations, between 22,000 to 26,000 years ago, corresponds with the time of the optimum of the Paudorf Interstadial. These occupations could just as well have occurred during the second and relatively severe half of the Paudorf Interstadial. Indeed it may well be that both levels are interrupted by cold oscillations. In any case, there is no evidence for a more severe cold interruption during the time between 26,000 years B.P. and 29,000 or 30,000 B.P. in Middle Europe—i.e., between the time of the earliest Gravettian sites in Central Europe and the period when there was no ice at the 1,700 m. level in the Alps and when Glütschtal lignite was formed.

It seems likely that the Paudorf interval was a relatively long, wet, mild or warm period lasting at least 10,000 years, the optimum of which is older than most Gravettian sites in Central Europe. Therefore we see no reason why it should not be regarded as a true interstadial. Some younger Aurignacian (*s. stricto*) sites seem to occur in the first half of the Paudorf Interstadial.

Oscillations between the Göttweig and the Paudorf Interstadials

In the Czechoslovakian loess sections other levels are known, which are characterized by the lack of sedimentation and the development of feeble soils. One of these levels is widely-spread and relatively significant. At present we cannot give further details concerning the origin of these soils; however, all of them point to an especially wet climate in which peat formation can easily be expected.

Some additional remarks

Our profile differs in detail from the older descriptions, due to the fact that the older scientists had no opportunity to clean and to study in detail the whole section. Furthermore, at the edges of our drawn profile, the layers that appear 35 meters to the north, above the abandoned wine cellar, are marked. Here the second, clearly developed "Verlehmungszone" underlies the middle chernosem in just the same way as at Sedlec (Prošek and Ložek 1957) near Prague.

Two other important facts have been discovered during recent months at the Dolní Věstonice brickyard:

a) After having washed 70 samples from the main profile, representing

more than 2 tons of loams, Ložek found some fragmentary remains of an interglacial malacofauna: *Helix pomatia* L., *Cepea vindobonensis* (Fer.) and *Monachoides incarnata* (Müll.). The remains were found at the base of the oldest chernosem and also in the rewashed loesses above this chernosem. This means that the basal lessivé is without any doubt of interglacial age.

b) Some 30 meters to the southwest of the main profile where the radiocarbon-dated samples were collected, an artifact made of Moravian Cretaceous flint was found, resting just on the surface of clearly developed humus soil above the Stillfried A Complex. (Depth: 10.50 meters in terms of the main profile.) The artifact is of Upper Palaeolithic type and is made of the raw material which is very often used, especially at Moravian sites from the older phase of our Aurignacian.

Summary: The new radiocarbon dates presented in our brief resumé show that as yet there is no necessity for abandoning the classical system for subdividing the Last Glaciation into 3 stadials, for all reliable European radiocarbon dates can be easily explained in terms of this scheme. [Prague, 10.12.60]

By H. MÜLLER-BECK

The discussion initiated by the Movius article touches upon one of the most difficult problems that Pleistocene archaeology faces today. The source of most of the difficulties (unusually enough) is the large amount of data available in this field. There are also many very different terminological systems, which compound the difficulties by using the same names for things that often are totally different.

Viewing the situation objectively, it is at present one in which one or another body of workers, each with its own terminology, tries to impose its own particular system throughout Europe. However, although it has largely remained unnoticed—at least officially—in the general confusion of the discussion, there has been some real *rap-prochement* in points of view in the last few years, occasionally even to a greater extent than many investigators may realize.

It is impossible here to open again a general discussion of the classical terminology. However, some observations (presented in more detail and with references in Müller-Beck 1959) must be briefly repeated. The basic foundations were laid by A. Penck and E. Brückner (1901-1909) and were based on the glacial formations of Upper Pleistocene

age in the Alps and especially in the *Alpenvorland*. W. Soergel, on the other hand, began his investigations (1919, 1924) with the Upper Pleistocene periglacial area in Central Europe. In contrast to Penck and Brückner, who believed they saw a rather uniform Upper Pleistocene glacial sequence, Soergel found phenomena which he interpreted as evidence of marked oscillations within the same sequence. Influenced by the conclusions of Penck and Brückner, Soergel began to look for evidence of more extensive advances of the ice-sheets, extending over the basin of the Baltic Sea and beyond the Alpine valleys, which he could combine with his periglacial evidence. This tendency, an unavoidable result of the general knowledge of that time, became even more pronounced after the publication of Milankovitch's theories (1930, 1941). The identification of "Warthe" with "Würm I" (in Soergel's sense) seemed satisfactory as a general solution for the problem involved. Attempts to correlate end-moraine stages in the *Alpenvorland* with Soergel's "Würm I" gained some acceptance, but were opposed by the most experienced field-workers in the Alps—C. Troll (1924), for instance, and Penck himself (1938). As we well know today, the "Warthe"—which very possibly is the same as the relatively well-established "Junggriss" (H. Graul 1955)—should be placed before the Eem Inter-glacial. Since the position of these stages has been clarified (some time before any substantial radiocarbon dating of Pleistocene sediments began), no major ice advance is known that could belong to Soergel's Würm I. Subsequently, the relatively late date of the glacial formations providing evidence of Würm advances in the *Alpenvorland* has been established by C-14 measurements (E. Ebers 1960; and several dates from still unfinished field work in the area of the Aare Glacier in Switzerland). But there remained much of Soergel's evidence in the periglacial area which could quite well be placed earlier than the glacial-stage of Penck. Further evidence of cool, but not too extreme, and also rather short oscillations preceding the maximum stages of the Würm (Penck) was presented, free of any doubt, in the comprehensive articles of K. Brunnacker (1953, 1957), which today are still comparatively unknown—perhaps because the author used a highly specialized method. However, this evidence demonstrated exactly the same situation as that stressed by H. E. Wright (1960) in his comment on Movius' article: a relatively wide gap between the true interglacial phase and the glaciomorphological Würm of

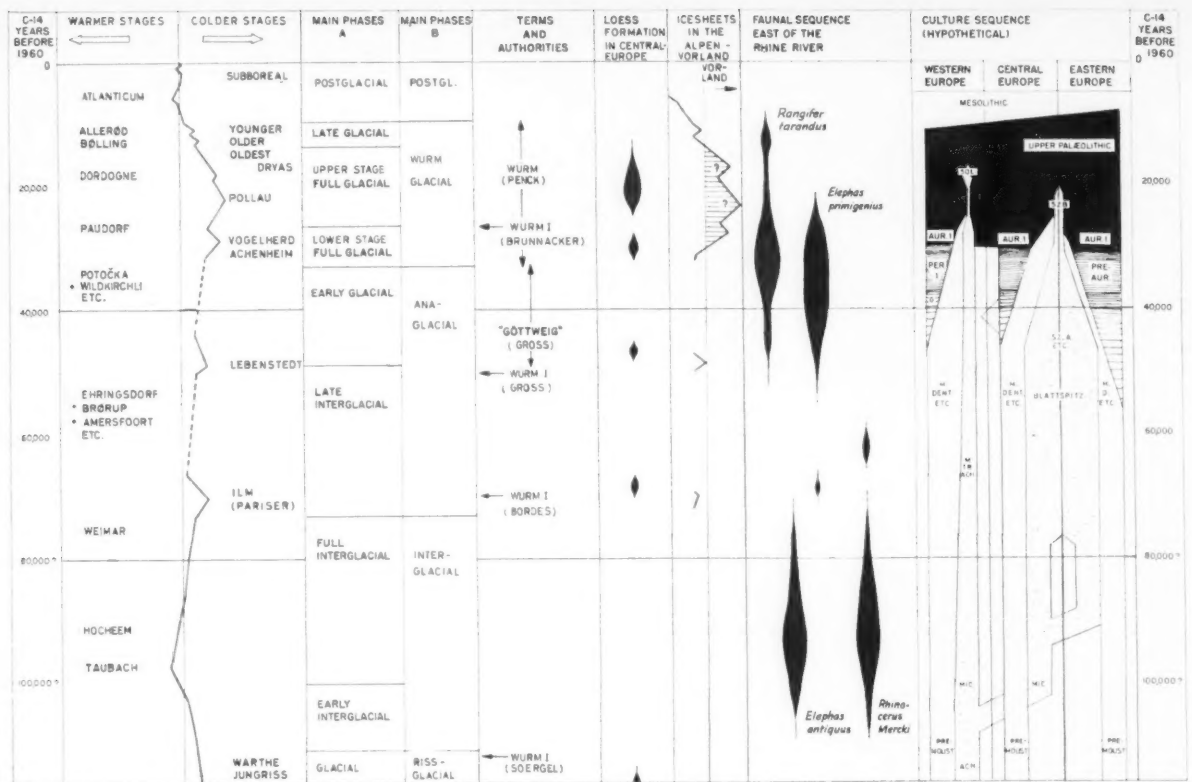


FIG. 2. MAIN STAGES OF UPPER PLEISTOCENE IN WESTERN AND CENTRAL EUROPE

Dates are given only as averages in radiocarbon years; those before 60,000 B.P. are estimates. Oscillations between cold and warm are relative only.

Abbreviations in the Culture Sequence column: Ach. = Acheulian; Premoust. = Premousterian; Mic. = Micoquian; M. tr. Ach. = Mousterian of Acheulian tradition; M. Dent. = Denticulated Mousterian; Blattsp.

= Blattspitzen (facies Mauern, Ranis, etc.); Sz. A. = Szeletian A (Sz. with mousteroid background); Preaur. = Preaurignacian; Pér. O and I = Périgordian 0 and 1; Aur. I = Aurignacian 1 (in a somewhat more general sense than used in France); Sz. B. = Szeletian B (Szeletian with Upper Palaeolithic background); Sol. = Solutrean.

Penck and Brückner. In this connection, we can find exactly the same opinion expressed by Penck and Brückner as early as 1909 (for details, see H. Müller-Beck 1959). During this time the conception of a "transitional stage," under the term *Forstossphase* ("advance phase"), was conserved by some authors in the futile discussion over the "Würm problem." It was used especially by palaeontologists (e.g. H. G. Stehlin 1922). Also, the writer (H. Müller-Beck 1957) was forced to use the term "transitional stage" in a revision of the Late Lower Palaeolithic industries from Germany. There simply was no evidence for a stronger relationship, in Soergel's sense, between the end of the full interglacial and the Alpine moraine stages.

If we accept (as did Soergel in the end) the terminology of Penck and Brückner, which has the right of priority, it is necessary to locate a datum from which it would be possible to bridge this gap between Interglacial and glacio-morphological Würm (Penck and Brückner). Most of the terms used have

2, 3, or sometimes even 4 different meanings, which are fixed in one or another way, and hence are a sufficient cause of confusion. It would be nothing but sheer optimism to think that clarification of this terminology would be possible in a short space of time. The true extent of this confusion, which prevents any understanding by someone who is interested in discovering the real picture, but is somewhat distant from the details of the evidence, can be easily shown by the very different uses of the term "Würm I" in the chart reproduced here (Figure 2). In the end it may be better to discuss the importance and the magnitude of the oscillations involved. Some workers see these as large, while others visualize them as very small.

It is impossible to discuss the Last Glacial stratigraphy productively without going back to the "Warthe-Jungriss" problem. The observations of F. Bordes (1954) in northwestern France provide clear evidence on the stages preceding the Last Interglacial (Ecm), and are far more important than any hy-

pothesis about the "Pre-Göttweig" constructed from the Central European loess sequence.

We may leave aside and unresolved the question of how cold were the initial temperature minima during the transitional stage. All the evidence now indicates that temperatures then never fell below values identical with those of the postglacial Boreal. This means, however, that the separation of Interglacial and Glacial becomes, at least to some extent, a terminological problem. Penck and Soergel thought that the "Glacial" would begin when the advancing Alpine ice masses extended into the *Alpenvorland*. There is no longer any doubt about the relative uniformity of these advances (R. Hantke 1958; J. Büdel 1960); consequently, any of the end-moraine stages belonging to this uniform formation are practically useless for stage differentiation of the Upper Pleistocene (in the general sense of this term). Nevertheless, it is probably possible to assign part of the transitional stage (or "Anaglacial") to the In-

terglacial, and part to the Glacial (H. Müller-Beck 1959).

In Central Europe no thick loess deposits have been found which can, with any security, be correlated with "Würm I" (in Gross's sense). All loesses belonging to the equivalent of Gross's "Würm I," whose position may be fixed stratigraphically, seem relatively thinner than is to be expected for a long-lasting uniform cold stage. To settle this question, to reach a more refined differentiation of climate, proof is required from radiocarbon dating and from artificial or faunal evidence, of the existence of a thick Central European loess deposit which can be attributed only to "Würm I" (in Gross's sense).

There are also certain difficulties in the use of radiocarbon dating for the Upper Pleistocene. This new technique has been basically very useful. It must be stressed, however, that the normal statistical error (provided by empirical evidence for a single determination) must be considered insufficient for chronological problems (H. Müller-Beck 1961). It would be better to use time-ranges which are at least twice as large as the error. If this is done, a large number of isolated dates have such a wide possible time range as to limit their value. Moreover, De Vries (1958), Willis, Tauber, and Münnich (1960), and Suess (1960) have observed that the concentration of radiocarbon in the atmosphere depends on changes in temperature. If this view is correct, allowance must be made for significant distortions in the radiocarbon time scale, especially during the transition from warm to cold, and from cold to warm. Here is a phenomenon which could easily create misinterpretation of chronology (H. Müller-Beck 1961).

A strong warning must also be given against the use of radiocarbon dates without publication and clarification of the stratigraphical evidence at the particular site. An example is the measurement from the Glütschtal exposure (B-20), which has already become important in these discussions (Gross 1959, Movius 1960a). In this case some material was dated which was collected by the late P. Beck more than 20 years ago. The exact stratigraphic position of the sample in the very complicated sediments of the valley is totally unknown. There exist at least 3 different layers of *Schieferkohle* (lignite) between the different gravels in the Glütschtal valley. A restudy (still unfinished) of the sections indicates a probable time-difference of some 10,000 years between the lignite layers.

After this general introduction, we may turn our attention to the present state of knowledge in Central Europe. Despite the considerations mentioned

above, observations which have already been made there permit some general reflections about the subdivision of the Upper Pleistocene and the importance of the different stages. It is, of course, impossible to discuss in detail the evidence available. We shall try to give only an outline of the more important main stages and the more important evidence. Somewhat more material bearing on this question can be found in another paper dealing with the same group of problems (H. Müller-Beck 1959).

At the very beginning of the Upper Pleistocene, in our opinion identical with the beginning of the Riss-Würm Interglacial (Eem), the most reliable evidence in Central Europe is provided by the sections from Taubach, near Weimar in Thuringia. The sediments there seem to fall mainly within the fluctuating Early Last Interglacial (G. Behm-Blancke 1960), but apparently also include the Eem maximum. Micro- and macrofaunistic remains correspond throughout with this interpretation. The only artifacts reported comprise a very primitive flake industry (G. Behm-Blancke), which belongs roughly to the same group as the slightly older material from Murg (H. Müller-Beck 1957)—a complex which, in general, is more or less identical with the Premousterian (in Obermaier's sense). To simplify the following discussion, we shall call the beginning—and the climatically more favorable part of the Main Eem ("Hocheem"), limited to Central Europe—the *Taubach Phase*.¹ This phase succeeds the "Junggriss" ("Warthe"); it also includes the beginning of the Interglacial as well as the Interglacial Maximum.

The next stage may also best be seen at the moment in the region of the Weimar travertine quarries (for further details, see H. Müller-Beck 1959). This stage occurs in the somewhat younger travertine layers from Ehringsdorf and Weimar (G. Behm-Blancke 1960). In the lower layers, with a climate slowly growing colder, there is still a typical warm fauna; it disappears after the onset of an initial colder stage in which appears the first mammoth (*Elephas primigenius*), but in which *Rhinoceros mercki* still occurs. The Lower Travertine from Ehringsdorf apparently includes the last part of the Full Interglacial, in a more restricted sense: it is a period, still relatively warm on the whole, but with a slow deterioration of climatic condi-

¹ It should be stressed that this "phase-name," like those that follow, is used only to make things more easily understandable, but is not part of any established or proposed terminology.

tions. For this part we may use the name *Weimar Phase*. The more extreme deterioration of the climate in the so-called "Pariser layer," which must be considered a true cold oscillation of importance, we may call the *Ilm Stage*. Thus, we avoid the misleading (and incorrect) name "Pariser," which is nothing more than a misspelling of the German word *Poröser* (from the adjective *porös*—porous). The following warm stage, in which *Rhinoceros mercki* still exists, may be called the *Ehringsdorf Phase*. During the Weimar and Ehringsdorf Phases there were already some leaf-point ("Blattspitzen") types (G. Behm-Blancke 1960). Whether in Central Europe at the time of the Ilm Stage some larger amount of loess was already deposited (as Bordes 1960a points out occurred in northern France under conditions that were probably special; H. Müller-Beck 1959) is so far unknown.

The subsequent sequence of the Upper Pleistocene cannot be traced clearly and directly in the sections of the Weimar region, which have been so very important for our knowledge of the Last Interglacial. Moreover, there is no clear connection of these sequences with any well-delineated younger section elsewhere. In the future perhaps a direct connection may be found with the help of radiocarbon dates. At present, the extent to which the Ehringsdorf Phase could be synchronized with the Brørup or Amersfoort Interstadial (Andersen *et al.* 1960) is totally unknown. It seems better, for the moment, to place these stages together into a *Late Interglacial/Interstadial Complex*. In this case the term "Interstadial" is used in a traditional sense; indeed, "Temperate Oscillation" would be a better term. On the other hand it seems to me permissible to unite these as the last part of the Interglacial. (There seems to be a contradiction between Interstadial and Interglacial; but, we are quite well accustomed to the use of the term Interstadial within the range of a Glacial.) J. Büdel (1960) has expressed some doubts about the possibility of using the term Late Interglacial for this stage-complex. However, I think there are at least two reasons for doing so:

1. Large sections of this phase, with only a small number of colder minima (which seem to be better marked in the coastal region of the Baltic Sea), appear at least similar to the climatic conditions of the warmer stages of the postglacial Boreal and Subboreal.
2. The introduction of the term *Late Interglacial*, together with the established term *Early Glacial*, gives a truer impression of conditions during the long transitional stage of the "Anaglacial" (H. Müller-Beck 1959).

We can now establish the onset of a

more pronounced cold stage from observations at the important site of Salzgitter-Lebenstedt (A. Tode 1953). From the general faunal sequence it seems relatively unbelievable that the *Lebenstedt Cold Phase*, (as we prefer to call this stage) is identical with the *Ilm Phase* (H. Müller-Beck 1959). The animals hunted at this site already show pronounced subarctic features; and the artifacts, aside from some general Mousterian traces, suggest something like a very late Micoque-tradition and some leaf-point (*Blattspitzen*) traces. A quite well developed bone-industry also occurs at the same site in the same layer. This site seems to fall just at the beginning of the cold stage; its radiocarbon age is given as 48,300 B.P. \pm 2,000 years (GRO-1219) as well as 55,000 B.P. \pm 1,000 years (GRO-2083). According to Gross (1960), the GRO-2083 measurement, not previously published, should be "more correct." At the moment there is no objective way to settle this point. Some general considerations (H. Müller-Beck 1961) might even suggest that both dates could be equally correct. It seems best to be cautious, and for the moment to place the beginning of the *Lebenstedt Phase* somewhere between 56,000 and 48,000 (radiocarbon) years ago: it is a relatively large range of time, but in our opinion it is the only correct way to express without misunderstanding the radiocarbon result in this case; and it affords a rather good chronological estimate.

The end of the *Lebenstedt Phase* is inadequately known. It looks as if we have to account for another Interstadial Complex, the *Early Glacial/Interstadial Complex*. This is a well-differentiated stage which, with some simplification, was generally termed (and still is termed) "Göttweig" (H. Gross 1960a). That this complex is indeed well differentiated was previously made probable by the loess studies of K. Brunacker (1953) and J. Fink (1956b). Similarly, V. Ložek (1955) rightly warned against a too simplistic correlation of loess sections from different sites. To the same *Early Glacial/Interstadial Complex* belongs at least the last part of the late Mousterian occupations in the Alps ("cave-bear hunters"), as documented in the *Wildkirchli* and the *Schnurenloch* caves in Switzerland (E. Schmid 1958) and (to some extent) in the *Potočka* cave in Yugoslavia (S. Brodar 1938, 1939). The detailed equation of the different oscillations in the range of this *Early Glacial/Interstadial Complex* should be avoided until the details needed to do so are available. The *Hörmatung Interstadial* (E. Ebers 1960) might also be part of this complex. Without a detailed discussion of the observations at *Hörmatung*, it may be of interest to note

that, beyond the layers which are considered as traces of the Interstadial, no till (ground-moraine) was observed. The layers on top of those of the Interstadial do not seem thick enough to guarantee against the possible infiltration of younger organic material (a danger which, following my own experiences in the *Glütschtal*, may be considered very grave). The *Early Glacial/Interstadial Complex* cannot even now be exactly fixed chronologically. It seems to begin somewhere between 45,000 and 40,000 (radiocarbon) years B.P., and to end somewhere between 35,000 and 30,000 (radiocarbon) years before 1950. The dates published so far for this time-range (H. Gross 1956, 1957a, b, 1958, 1959, 1960a; H. L. Movius 1960a; Sv. Th. Andersen 1960; and others in the dating lists of the Radiocarbon Laboratories, especially Groningen) do not permit a more exact delimitation of it. The climate of the complex as a whole was, on the average, cool-boreal, perhaps sometimes warmer; and sometimes, in the colder oscillations, it was perhaps for a short time cool-subarctic. In the *Lebenstedt Phase*, loess formation seems to have been more general in Central Europe. It occurs under the strong influence of regional climatic conditions in regions more favorable for loess over a shorter time within the cool-subarctic oscillations of the *Early Glacial/Interstadial Complex*.

The next well-expressed cold stage of Central Europe is more clearly discernable; it is more or less synchronous with the so-called "Typical Aurignacian" and similar industries. Stratigraphically, this stage is not identical with the full glacial temperature-minimum, and seems to have possessed in France climatic conditions that were still quite favorable (F. Bordes 1960b). In Central Europe it is best seen stratigraphically today in the layers of the *Vogelherd Cave* near *Stetten* (southern Germany), and even west of the river Rhine at *Achenheim* (H. Müller-Beck 1959). For this reason this stage may be called the *Vogelherd-Achenheim Phase*. The first general loess deposition occurred in thick layers over large regions of Central Europe in this phase. This phase probably began even prior to 30,000, but perhaps not more than 35,000 (radiocarbon) years ago. It seems possible that the loess deposition itself lagged somewhat behind the climatic minimum of the *Vogelherd-Achenheim Phase* (H. Müller-Beck 1959).

The *Paudorf Interstadial* is stratigraphically well-documented in Central Europe. Reliable radiocarbon dates for the beginning of this Interstadial are still unavailable. For this reason it is impossible to give any estimate of its length. According to the presently avail-

able dates, it may be considered that all of the evidence for this Interstadial must be dated before 25,000 (radiocarbon) years ago. F. Bordes (1960b) quite rightly stresses the fact (which emerges clearly in Movius' article) that *Paudorf* is not very well expressed in the cave sediments of southern France; and the same seems to be more or less true of the caves of southern Germany (H. Müller-Beck 1959). This observation makes sense if the *Paudorf Interstadial* precedes the full glacial climatic minimum. If so, in the caves, especially those of southern France, differentiation of sediments should be less well marked than is the case in the loess regions. A more marked difference in sedimentation in the more continental climate in the open loess areas of Central Europe is entirely possible in the same range of time. This point is seldom given sufficient consideration in discussions of likely regional climatic differentiation in Europe in the Upper Pleistocene. This phenomenon could account for exact observations which seem to be contradictory, and would make it unnecessary to persist in the idea of a single general stratigraphy for all of Europe. The importance of regional climatic conditions was theoretically pointed out several years ago (F. Bordes and H. Müller-Beck 1956), but even then there was the convincing evidence afforded by J. Fink (1956a) and K. Brunacker (1956a) for several relatively small neighboring areas.

The next cold stage is well documented from sites overlying the *Paudorf Interstadial* in the loess regions of Central Europe. The sites of *Pollau* and *Unterwisternitz* are situated at the beginning of this apparently extremely cold stage; but, these are not really in the Interstadial in the strict sense of this term (H. Müller-Beck 1959). The beginning of this stage, which seems identical with the temperature-minimum in Central Europe, falls shortly before 25,000-26,000 (radiocarbon) years B.P.; we may call it the *Pollau Phase*.

At this point a short digression about Western Europe may be worthwhile. Everything points to the possibility (see Movius' article) of another smaller and warmer oscillation in southern France, falling shortly after the temperature-minimum. So far it has not been found in Central Europe, where deposits were formed under more continental conditions. F. Bordes (1960b) has already pointed out this oscillation in the range of the more evolved Solutrean. This oscillation, which we would provisionally call the *Dordogne Phase*, cannot yet be chronologically fixed. According to the dates now available, it seems to fall somewhere between 20,000 and 17,000 (radiocarbon) years ago.

Apparently this phase was followed, within the framework of a general tendency toward rewarming and more favorable conditions, by reversion to a relatively colder climate. There was possibly more general loess deposition in the Oldest Dryas, dated between about 17,000 and 13-14,000 (radiocarbon) years B.P., in Central Europe; very probably this stage should not be considered entirely uniform. It was followed by the Bølling Interstadial, which is now well-fixed in the radiocarbon chronology. Apparently only in northern Europe can the Bølling Interstadial be clearly separated, by an independent Older Dryas, from the Allerød. In Switzerland these 3 stages may fall into one scarcely subdivisible paleobotanic unit (M. Welten 1958). This is more or less comparable to the regional differentiation in the time of the Paudorf Interstadial in Central Europe and the Dordogne Interstadial in southern France.

About 10,000 (radiocarbon) years ago the Late Glacial ends and the Postglacial begins. In Central Europe this time is identical with the end of the Palaeolithic and the transition to the microlithic Mesolithic (e.g. see the date of the transitional Fürstener site in Switzerland, *Jahrb. Hist. Mus. Bern* 1957/58 [1959], p. 170).

In summary we may note that:

1. The whole Upper Pleistocene time-range may be differentiated into 3 principal and more or less equivalent stages: Interglacial; Anaglacial (Late Interglacial and Early Glacial); and Glacial.

2. The climate, in spite of numerous still ill-defined oscillations of variable magnitude, passes from the warm climatic maximum of the Last Interglacial to a single cold climatic minimum (between about 25,000 and 20,000 [radiocarbon] years ago).

3. Regional climatic differences must be recognized in Europe, especially before and after the relatively short climatic minimum (Pollau Phase).

All currently available evidence suggests that the great ice-advances into the *Alpenvorland* occurred no earlier than 35,000 to 30,000 (radiocarbon) years ago (H. Müller-Beck 1959); the maximum advances of the ice coincided perhaps with the Pollau Phase. The various retreats and minor readvances are perhaps to be correlated with the increasing temperature around the time of the Dordogne Interstadial and the subsequent oscillations up to the post-glacial Atlanticum (H. Müller-Beck 1961).

Certain mammals seem to be restricted to specific stages in Central Europe (east of the Rhine river). *Elephas antiquus* appears within the maximum of the Eem Interglacial, but is absent,

apparently permanently, by the end of the Weimar Phase and the beginning of the Ilm Phase. The warmth-loving *Rhinoceros mercki* (= *kirchheimensis*) appears at about the same time as the Forest-elephant, but the former survived in considerable numbers until the time of the Ehringsdorf Phase (H. D. Kahlke 1958). The first traces of *Elephas primigenius* (Mammoth) appear as early as the end of the Ilm Phase (G. Behm-Blancke 1960); subsequently the mammoth disappears from Central Europe and reappears only in a later stage of the Late Interglacial/Interstadial Complex. The main appearance of the *Elephas primigenius* seems to occur in the earlier part of the Full Glacial (Vogelherd-Achenheim); but, in most parts of Central Europe the mammoth seems to disappear relatively abruptly during the climatic minimum (Pollau Phase). The lack of appropriate plant food may have forced these large animals to emigrate into the somewhat warmer steppes to the southeast.

There are several consequences to the acceptance of the general succession outlined here. It is impossible here, of course, to present more than a brief summary comparing this succession with prehistoric interpretation based on Soergel's scheme, which still constitutes the foundation for almost any archaeological succession of the Upper Pleistocene in Central Europe. However, it is clear from the table (Figure 2) that the interpretation offered here is at variance with that of Soergel in substantial ways. In particular, two points should be noted:

1. Many major difficulties may arise if attempts are made to correlate sections from those regions with more favorable climatic conditions (e.g. the Near East or the Mediterranean coasts) with sections in Central or Western Europe. For example the Ilm Phase may be scarcely marked in the Near East, and the same difficulty may occasionally arise in delineating the Lebenstedt Phase. If a more detailed subdivision of Last Interglacial climatic phases is eventually arrived at, perhaps it will be easier to correlate movements of sea level in pre-Würm times. These were recently observed, with good control of morphological details, on inland beaches in the coastal region of Mallorca (C. Troll 1959).

2. A well established general stratigraphy and chronology of the Upper Pleistocene is essential for the study of human cultures. A usable stratigraphy and chronology are already available if one starts with the beginning of the Last Interglacial and is aware of the gaps in the sequence (e.g. the ranges of

the two Interstadial Complexes), rather than with an apparently practical, but incorrect, hypothetical formulation. This may be done considering 3 parts of continental Europe: (1) Eastern Europe (east of the eastern border of Poland); (2) Central Europe, from there to the Rhine; (3) Western Europe, from the Rhine to the Pyrenees.

In all 3 regions, a possibly autochthonous, simple, flake-tool industry seems to be already present by the beginning of the Eem Interglacial; following Obermaier, this might be best named "Præmousterian" (a term also used in France). A typical tool assemblage has been found at Taubach near Weimar (G. Behm-Blancke 1960). During the same period there is a well-developed late Acheulian (Micoquian) industry in France. This late hand-axe industry seems to penetrate Central Europe and then Eastern Europe with the onset of a more favorable climate during the Taubach Phase of the Last Interglacial (H. Müller-Beck 1957). The spread of forest-cover during the Interglacial probably brought about differentiation of particular vegetation regions in Europe; in these, geographically distinct cultural developments could have occurred. Cultural development in Western Europe is already quite different by the time of the Full Interglacial, compared with that in Central and Eastern Europe. The classical development of the various Mousterian industries in Western Europe has a relatively early counterpart in the leaf-point (*Blattspitzen*) industry development east of the Rhine; the latter begins as early as the Weimar Phase of the Full Interglacial, at least in central Germany (G. Behm-Blancke 1960). These cultural developments still seem to be separate in the Late Interglacial/Interstadial Complex. During the Lebenstedt Phase, and also after it, climatic changes seem to have allowed greater cultural diffusion between the previously isolated regions. There is some trace of the leaf-point (*Blattspitzen*) complex in southeastern France; and there is also more rapid change among the different Mousterian industries in various parts of southern France. East of the Rhine the leaf-point (*Blattspitzen*) industries culminate in the subgroups of Mauer (Szeletian A, generally Mousterian in facies) and Ranis. About the same time there seems to be more extreme differentiation of the Mousterian industries in Western Europe. The first appearance in Europe of the true Upper Palaeolithic might be looked at in a new way. An Upper Palaeolithic developmental trend is perceptible in Eastern and Central Europe as well as in Western Europe; in the

latter area this is represented by the Périgordian 0 and 1, and in the former by the Pre-Aurignacian industries (in the sense of A. Rust, and including the Olshewian as established by K. J. Narr 1955). The use of bone tools, one of the main features of the Upper Palaeolithic trend, generally seems to be older in the area east of the Rhine; very well-made bone implements are found there at a relatively early period (long ago recognized and stressed by O. Menghin). There are the primitive bone implements from Ehringsdorf (G. Behm-Blancke 1960), as well as the very finely-worked and more specialized bone implements from Lebenstedt. There are also a great number of very refined bone tools in the entire area east of the Rhine during the Early Glacial/Interstadial Complex. Nothing like this richness is to be seen in France in this period, but in Western Europe an independent trend toward a true Upper Palaeolithic stage can be seen in the stone industries even before the first traces of the Aurignacian (as Bordes [1960b] has already noted). After the appearance of the Typical Aurignacian, the over-all culture of Europe is generally very similar. This would seem to be possible only at a time when there already were indigenous "Upper Palaeolithic roots" in Western Europe. Only under such conditions, with contact between a fully established Upper Palaeolithic group (Aurignacian I), and a cultural manifestation of nearly equal development (the Périgordians 0 and 1), does a sudden cultural unification seem at all possible in such a very brief time. If this is the correct interpretation, and it looks likely from present knowledge, this cultural unification would resemble the mechanism of inheritance discovered in animal- and plant-breeding. Alternatively, it is necessary to revert to an old and still common belief that the peoples of the Mousterian in Western Europe were "exterminated by the more highly evolved Upper Palaeolithic people," somewhat in the manner of a super-race. This theory affords a relatively simple explanation, but one which the writer believes fails to account for all the evidence. A more detailed discussion of this very interesting question is impossible here, but aside from other facts, the reappearance of the Périgordian traditions speaks against this hypothesis. Moreover, it is easier to understand the background of cave-painting in Western Europe if we have something like a mixture of conservative Old Palaeolithic traditions with newly formed Upper Palaeolithic improvements. Finally, an extermination of this magnitude is contradicted by everything known about the behavior and customs of hunting peoples and their war-like

capabilities. This absurd theory is a rather old-fashioned simplification derived from the super-race theories of the 19th century.

Industries of more-or-less well expressed Lower Palaeolithic traditions continued to exist for a long time alongside the Upper Palaeolithic groups throughout Europe. A late representative of these in Eastern Europe is the Szeletian B; it is, in addition to leaf-points (Blattspitzen), an industry of Upper Palaeolithic type. The Upper Palaeolithic of Western Europe also preserved older technological traditions; these seem to reappear in the French Solutrean, which is independent of the leaf-point (Blattspitzen) industries of the area eastwards of the Rhine. The classical Solutrean, as F. Bordes has pointed out, seems to represent a combination of very late hand-axe tradition with some other Mousterian traditions and true Upper Palaeolithic improvements (a possibility recognized by G. Freund 1952: 287) rather than a direct influence of the leaf-point (Blattspitzen) industries of Central and Eastern Europe. The progressive deterioration of climate in the Full Glacial seems to have brought about a cultural partition in Europe, with western and northern Central Europe coming to resemble something like a no-man's-land. Only the Late Glacial climatic improvements interrupted this division for a rather brief time. Movements over larger areas ensued, with many different influences manifest among the various industries throughout Europe, ultimately to end with the growing isolation of the Mesolithic. [Bern, 1.3.61]

By KARL J. NARR

Movius' article broadly follows the lines of Soergel's subdivisions of the Upper Pleistocene. This scheme has been adopted by most archaeologists, primarily because Soergel did much work on periglacial sediments containing archaeological material, and correlated his system with the dating of Palaeolithic complexes to a far greater extent than did Penck. Yet, Soergel's scheme has often been questioned. Today, an increasing number of scholars, especially geologists and pedologists engaged in continuous field work in the type regions of Austria and Southern Germany, no longer accept its validity. Although far from being uniform in all details, their objections share a core of features which clearly indicate that the subdivision of the Upper Pleistocene needs thorough reconsideration. This re-evaluation is particularly needed for the time before the Main Würm Glaciation; or, according to radiocarbon dates, before $29,000 \pm 1,000$ B.C.

The sequence of periglacial sediments—especially the loesses—and the formation of soils shows marked differences with respect to the local climatic conditions. This has been best established for Bavaria and Austria (Brunnacker 1956a; 1957; Fink 1956a; 1956b), but is also true of other regions (Fink 1956b; Brunnacker 1958). Although details of the correlations are still disputed, some main lines of common development are already recognizable (Fink 1956b; Brunnacker 1958). The Götting loam horizon and corresponding soil formations are followed by periglacial phenomena, especially by relatively thin loess layers and basal solifluctions, and then (proceeding from "drier" to more "humid" climatic provinces within different regions) by humus horizons with intercalated thin loess layers, "wet" tundra soils, or solifluction layers, which in Austria form the upper parts of the Stillfried Complex ("dry type") and Linz Complex ("humid type"). These layers above the Götting loam (the type site of which belongs to an "intermediate type") and its correspondents represent an "Early Glacial Period" (Brunnacker 1957; Fink 1956b), whose importance has especially been stressed by J. Büdel (1950). It can be subdivided into a time of increasing cold (Brunnacker's "FWa") reaching a first cold climax (Brunnacker's "FWb"), followed by a time of climatic amelioration (Brunnacker's "FWc").

Calling the upper parts of the Stillfried and Linz Complexes and the corresponding layers of other regions "Götting," and thus regarding them as upper parts of an extended "Göttingian," may seem a mere terminological matter. But in reality, this denomination usually expresses the view that the humus layers, the "wet" tundra soils, and their correspondents, lying above the Götting loam and its correspondents, belong to the same single more-or-less uniform period of soil formation as the Götting loam itself. That the separation of those complexes into a true Götting Period and an "Early Glacial Period" has only recently been recognized is partly due to the fact that Early Glacial actions, especially solifluctions, often destroyed the genuine soils, and the soil materials are embedded in the solifluction masses (see especially Freising 1951; how these soils, by their very nature, fostered solifluction has recently been shown by Büdel 1959: 302). Thus, Early Glacial layers are often misunderstood as true Götting layers.¹

¹This is also true of cave deposits, in which abrasion played a greater role than has hitherto been recognized (see Brunnacker 1956b and Müller-Beck 1957). This is, naturally, more often the case with still

Furthermore, the whole question is almost irreparably obscured by terminological confusion. At a small *ad hoc* conference, the term "Würm I" turned out to be understood in seven different senses (see Fink in Felgenhauer *et al.*, 1959: 45). Whether the "Würm" is a relatively uniform period, or at least a period undivided by soil formations of the Göttsweig order (Büdel 1950), so that consequently the Göttsweig Period must be regarded as "Riss-Würm" (Brunnacker 1957, 1958; Fink 1956b; Weidenbach 1955), is still much discussed (see also Narr 1951, 1953), and is also partly a terminological problem. Therefore, since 1953, I have used the expression "*Letzte Warmzeit*" ("last warm period") of the Pleistocene, deliberately setting aside such questions as whether this is "inter-Würm" or "pre-Würm"; and whether it is a "great" and/or intense "interstadial," or an "interglacial" shorter and or less intense than the preceding interglacials. The definitions of "interglacial" and "interstadial" are rather arbitrary, and differ with respect to the chosen criteria: extent of glaciation, duration, fauna, flora, and soil formation.

As to the climatological side of the question, it must be stressed that the palaeontological evidence used to establish the relatively "cool" or "boreal" climatic character of the Göttsweig Period is taken almost entirely from layers belonging to the succeeding "Early Glacial Period" and especially to its interval of climatic amelioration. For the true Göttsweig Period, the evidence still rests on predominantly pedological deductions, which seem to favor the view that it was a rather long period of relatively warm climate (Brunnacker 1957; Fink 1956b).

Calling the Göttsweig Period "Riss-Würm," however, would not directly imply that it can be identified with layers commonly regarded as "Riss-Würm" and containing the last faunal assemblages with *Elephas antiquus* in Central Europe (Brunnacker 1957), or that it forms a younger part thereof (Müller-Beck 1959). It seems more probable that layers with *Elephas antiquus* (as at the famous sites of Ehringsdorf and Gánovče) are older than Göttsweig and separated from it by a cold stage, during which the Lower Young Loess ("Young Loess I" of Soergel; "Loess II" of Bayer and Freising) was deposited. This is called "Würm I," "Old Würm," or "Early Würm" by many scholars, but others (Brunnacker 1958; Müller-Beck) older cave deposits, as is clearly shown—despite different opinions in detail—by the tables of Müller-Beck (1957) and Prošek and Ložek (1957): layers containing faunal assemblages with *Elephas antiquus* are also very rare elsewhere compared with those of later times.

1954/55; Weidenbach 1955) correlate it with a "Jung-Riss" of the Alpine region (see Graul 1951, 1952, 1953). However, this is beyond the scope of the present comment.

In the Austrian type region, we now have available the valuable radiocarbon dates from Oberfellabrunn and Willendorf cited by Movius. It must be stressed, however, that samples 3–5 from Oberfellabrunn were taken from the humus layers above a loam obviously corresponding to the Göttsweig loam. Moreover, their real age may well be higher than the radiocarbon dates (De Vries, in Felgenhauer *et al.* 1959: 72). The radiocarbon dates for the Aurignacian of Willendorf are related to humus layers with traces of solifluction, which have therefore been called "Early Glacial" (Narr 1955: 125). The view that they were deposited after the close of the Göttsweig soil formation has recently been further substantiated by Brandtner in the Willendorf monograph (Felgenhauer *et al.* 1956–59). To sum up: Dates between 40,000 (or more) b.c. and 29,000 b.c. from the Austrian type region fall within the "Early Glacial Period," which follows the Göttsweig soil formation in time and must be distinguished from it. Furthermore, there is no proof, or even probability, that the date of about 40,000 (or more) b.c. marks the beginning of the "Early Glacial Period;" the dates between 40,000 (or more) b.c. and 29,000 b.c. may well belong to the interval of climatic amelioration which forms only the upper part of the "Early Glacial." However, some deductions about the beginning of the Early Glacial interval of climatic amelioration, the "Early Glacial" as a whole, and the end of the true Göttsweig Period, can be made when taking into account dates from outside Austria.

The Hörmating section was presented by E. Ebers to the meeting of the Deutsche Quartärvereinigung in June, 1960. Ebers' views (previously summarized in the C.A.* comment by Gross), were questioned by several participants. Brunnacker, in particular, stressed that the lower parts of this section represent the "normal sequence of Early Glacial events." This view is sustained by the radiocarbon date of 43,350 ± 1000 b.c., which probably can be connected with the weathered stratum on top of the "lower Würm gravels." Whether this date is really a *terminus post quem* for the beginning of the weathering period, and this can be guessed at as ca. 41,000 b.c., is by no means beyond doubt. We cannot exclude the possibility that the peat layer, from which the sample has been taken, may fall within that period

of weathering. In that case, the date of ca. 43,000 b.c. would be a *terminus ante quem* for the beginning of the weathering. Be that as it may, the weathered stratum on top of the "lower Würm gravels" belongs to a time of climatic amelioration, for which we already have dates between 40,000 (or more) b.c. and 29,000 b.c., obtained from layers above loam horizons corresponding to the Göttsweig loam. Even if one accepts the low guess for Hörmating, the end of the Göttsweig period should be placed considerably earlier than 41,000 b.c.

If we place the Mousterian layer of Lebenstedt at the beginning of a pre-Göttsweig cold phase, only a few thousand years are left for the deposition of the main lower Young Loess (Young Loess I of Soergel; Loess II of Bayer and Freising) and for the alteration of its upper part forming the Göttsweig loam. Thus, even the new date of around 53,000 b.c. would leave considerably less time for the true Göttsweig Period than for the Early Glacial interval of climatic amelioration! That seems rather unlikely. Moreover, such a placing of Lebenstedt is unnecessary. The Mousterian layer has been partly affected by periglacial actions, and is covered by two solifluction layers. The strata between the solifluction layers contain a flora indicating a certain climatic amelioration (*Salix* and *Betula*, for which one must bear in mind that the site lies near the margin of the north German lowlands). It is significant in this respect that the layers from sites in the Netherlands whose radiocarbon dates fall between about 41,000 b.c. and 30,000 b.c. are also regarded as "Pleniglacial" (De Vries 1958). This, too, may well represent the sequence of "Early Glacial events" worked out in the south German and Austrian type regions. Placing the Mousterian layer of Lebenstedt at the beginning of the Weichsel Glaciation (Preul, in Tode *et al.* 1953) cannot decide its relation to south German or Austrian phenomena, for it is quite uncertain whether the Northern Weichsel Glaciation equates with the "Würm" in the larger sense (including the Göttsweig loam), or—more probably—only with the cold phases following the Göttsweig loam. Having examined the archaeological material, I cannot share the view that the Mousterian of Lebenstedt must be "old." That view seems to rely on the long-outdated evolutionary reconstruction, still repeated in some German textbooks, in which a Mousterian with hand-axes is automatically placed relatively early in the Mousterian sequence. Moreover, some bone implements and stone tools of rather "Leptolithic"

aspect may point to the opposite direction. There remains the problem whether the different radiocarbon dates obtained from two peat lenses near the base and in the upper third of the culture layer are due to the normal margin of error in radiocarbon-dating, to contamination, or to a relatively long duration of deposition.² Be that as it may, placing the Mousterian of Lebenstedt at the very beginning of the "Early Glacial" and/or at the very end of the Göttweig Period would give a reasonable space of time for the development of both periods. Moreover, this placement would match the date of more than 52,000 B.C. from Senftenberg—provided that Fink is right in assigning the sample to the top layer of a loam (see presentation of documentary evidence in Felgenhauer *et al.* 1959: 58–60).

To sum up: the evidence available today favors the view that the true Göttweig Period should be placed considerably earlier than the interval of climatic amelioration between 41,000 (or more) B.C. and 29,000 B.C. The guess date of "around or after 50,000 B.C." for the end of the Göttweig Period (Narr 1959) was based on the old radiocarbon dating for Lebenstedt (and, with reservations, Senftenberg), and should be changed to "around, or more probably before, 50,000 B.C." on the basis of the new datings. Of course, many questions remain. But the early dating of Göttweig should at least be borne in mind as an alternative solution (Narr 1960).

Of course, this does not affect the absolute dates of the Châtelperronian (or "Early Périgordian") and Aurignacian of France and the main Aurignacian layers of Central Europe. Nevertheless, the splitting-up of several complexes of soil formations, which I had not yet perceived when establishing my 1955 scheme, entails partial reconsideration of the relative archaeological sequence (including parts of the scheme of Narr [1955]). Furthermore, much has been obscured by disregard of abrading and emptying of cave deposits by Early Glacial events (for southern Germany see Brunacker 1956b; Müller-Beck 1957).

In the Istállóskő Cave, the lower brown loam (containing split-base bone points associated with a rather insignificant stone industry) is separated from the upper brown loam (containing the so-called "Aurignacian II," with Lautsch [Mladeč] bone points and

² It should be mentioned that the lower peat lens contained much *Salix*, whereas *Salix* is absent in the upper lens; but the other plants from the upper lens suggest conditions more humid than those of the lower one (Pfaffenberg in Tode 1953:164.)

stone implements showing a more "Aurignacian" style) by an abrasion which left only a few lenses of a reddish layer. Between the two loams, moreover, lies a "gray rubbish," also containing split-base bone points, from which the sample for the unreliable radiocarbon dating GRO-1501 was collected (Vértes, *in litt.* April 11, 1960).³

This "gray rubbish" (and therefore the upper horizon of Vértes' so-called "Aurignacian I") probably belongs to a period of climatic amelioration, to the end of which can be assigned the loam containing the archaeological assemblage which Vértes called "Aurignacian II." The latter has traits suggesting that it was in contact with the oldest horizon of Gravettian elements in Central Europe (as supposed by Narr 1955: 129; 1959: 148), and consequently was roughly contemporaneous with, or even slightly later than, Willendorf II/5, as now confirmed by the radiocarbon date GRO-1935. The "gray rubbish" was most probably deposited after the abrasion took place, although its relation to the remnants of the red layer is not yet clear. (Vértes *in litt.* April 11, 1960). The gap in the sedimentary record caused by this abrasion—which most probably separates the uppermost horizon of the so-called "Aurignacian I" (in the "gray rubbish") from the lower loam containing, from top to base, the lower (and main) parts of the so-called "Aurignacian I"—may be due to the initial cold phase of the "Early Glacial Period." One may doubt that the assemblage with split-base bone points is truly "Aurignacian"; but this is primarily a matter of terminology, and a minor problem in respect to the stratigraphical and chronological position. If the abrasion actually occurred at the beginning of the "Early Glacial," the earliest occurrence of the split-base bone points (at the base of the lower loam: confirmed by Vértes, *in litt.* April 11, 1960) would fall within the Göttweig Period, and therefore would be many thousands of years older than the French Aurignacian! This raises the question whether the abrasion could not have been caused by purely local events independent of the general sequence of late Pleistocene climatic conditions. Such an assumption could allow a more recent date. However, a very early date for the oldest split-base points should not be discarded for purely a priori reasons.

The "Olschewian" assemblage of the Potočka Cave yielded a fine series of

³ The "gray rubbish" and the border zone between the two loams yielded the greatest portion of *Larix-Picea*, whereas *Fagus sylvatica* comes only from the lower loam.

Lautsch (Mladeč) points, but also one small point which differs from "classic" Aurignacian split-base points in shape and section, though it shows the basal splitting (Brodar 1938; Delporte 1958). The chronological equation of the "Olschewian" from Potočka Cave with the so-called "Aurignacian II" of Istállóskő Cave seems rather unfortunate, and has caused some unnecessary difficulties. A really satisfactory definition of the "Olschewian" can hardly be found; and its separation from the true Aurignacian of Central Europe can only be formulated by almost entirely negative characteristics (absence of the classic Aurignacian bone points, and the extreme rarity of the true Aurignacian retouch and of such very characteristic forms as the keeled scraper). One may rightly doubt that this is a sufficient basis for chronological equations, particularly if one admits the possibility that the Olschewian represents only a "lateral facies," due either to distinctive local conditions and activities (Narr 1954: 3; Schwabedissen 1956: 24, footnote 38), or to ethnic differences (Vértes 1955a)—a question that can hardly be answered. There is no really convincing argument against considering the main "Olschewian" layers of the Potočka Cave (layers G–H of section 2: see Brodar 1938) to be earlier than the so-called "Aurignacian II" of the Istállóskő Cave. They may well be placed in the Göttweig Period, whereas several higher layers (F–C of section 2), which yielded very poor vestiges of human activities, may belong to the "Early Glacial Period." The occasional frequenting of Alpine regions by man during the climatic amelioration within the "Early Glacial" is perhaps also indicated by the radiocarbon-dated finds from Salzofenhöhle (*ca.* 32,000 B.C.), whereas the equation of an older part of the "Alpine Palaeolithic" with the Göttweig Period may be supported by the date for Drachenloch (more than 47,000 B.C.; Schmid 1958).

This view implies the early occurrence of elements that later form part of the Upper Palaeolithic complexes. A long co-existence of different cultures, comparable to the intercalations of "Leptolithic" strata in the Near Eastern Levallois-Mousterian, is at least possible. The Balkan countries may produce some surprises in the future, of which the split-base bone point from Morovitz and the finds from Bacho Kiro (Garrod 1939), where layer J might well turn out to be as old as the lower layers of Istállóskő and Potočka Caves, are only very promising beginnings.

Be that as it may, there is no a priori reason why elements, which in later periods are regarded as "Leptolithic,"

should not occur in southeastern Europe and certain parts of Central Europe considerably earlier than in Western Europe and other parts of Central Europe (where eventually they may have influenced contemporaneous Mousterian groups).

Radiocarbon datings have now confirmed what archaeological comparisons had already made quite clear: that the latest Mousterian, the Châtelperronian (or "Early Périgordian"), and the earliest Aurignacian of France belong to the same period of climatic amelioration as the latest Mousterian and the bulk of the Aurignacian of Central Europe. This interval, which now can be said to form part of the "Early Glacial," must obviously be identified with the "Würm II/III" of Bordes. Consequently, the "Loess récent II" in all probability was deposited during the initial cold and the first cold climax of the "Early Glacial." Thus the Mousterian found on the base of that loess of northern France becomes roughly contemporary with that of Lebenstedt and Kitzingen (Brunnacker 1956c), which seems a fairly reasonable correlation.

This implies more extensive loess deposition in France than in Central Europe in "Early Glacial" times; but this effect may be due to regional differences. Bordes and Müller-Beck (1956: 204) have discussed some of the possible causes with respect to the assumed absence of Central European correspondences to the lower Young Loess of France; but the above equation reduces this assumed absence to gradual differences. Furthermore, correlation of Bordes' "Würm II/III" with the climatic amelioration within the "Early Glacial"—and hence, a probable correlation of Bordes' "Würm I/II" with the true Götweig, the vestiges of both being often difficult to find because they underwent heavy abrasion—greatly reduces the difficulties mentioned in Bordes' CA* comment. It also deviates from the correlation proposed by Bordes and Müller-Beck. But the improbability of the latter could already be discerned from the fact—obscured by the omission of local numeration in their terminology—that it equates Bordes' "Würm II/III" with Brunnacker's "Würm I/II," which in all probability corresponds to the horizon Paudorf-Stillfried B of the Austrian loesses (see also tables of Müller-Beck 1954-55: 170-71 and Brunnacker 1958: 146-47). This would chronologically equate the "Série mate" of Goderville with, or at least approximate to, the cultural layers of Pavlov/Dolní Věstonice! Hence the scheme of Bordes and Müller-Beck (1956) is also clearly incongruous with that of Valoch and Bordes

(1957) and obviously must be discarded.

Many problems remain for the more recent periods as well. There is no really satisfying correspondence to Paudorf-Stillfried B (and its probable equivalents, e. g. the "Sol de Kesselt" in Belgium) in the French loesses. Moreover, the "Périgordian V" level (of more "interstadial" character: see Bordes 1960b; also Narr 1955: 142), according to C-14 dates, should be placed later than Paudorf-Stillfried B. But there are also indications of a slight climatic amelioration during the Aurignacian II, III, and IV (see Movius' article; also Narr 1955: 136) and another one ending approximately with the Magdalenian III (see Bordes 1960b). Perhaps we are dealing with a rather long period of slight climatic oscillations which widely maintains the "thermoclastic" nature of cave deposits and is inserted between two cold climaxes. Paudorf-Stillfried B may represent only a relatively short part of that period, the other parts of which are not distinguishable in loess deposits and in the cave sediments of less maritime Central Europe (see maps by Büdel 1949, 1951; and Poser 1947). The local or regional conditions, probably different interactions of hygric and thermic components of the glacial climate, still remain to be clarified for the most part. The climaxes of cold climate of what is called "Middle Würm" by Movius and others, however, must have been very short, and ought to be placed after ca. 28,000 B.C. (i.e., after the already severe cold of the main Aurignacian I period, which in many caves is followed by "sterile" thermoclastic layers consisting of large elements with predominantly sharp angles) and immediately before, or perhaps around, 14,000 B.C.

As to the latter figure, it is highly improbable that the "Oldest Dryas" can be equated with that cold climax. The "Oldest Dryas" obviously marks the beginning of late glacial conditions, during which a tundra vegetation occupied the Northern lowlands roamed by the Hamburgian hunters in the favored seasons. The "Oldest Dryas" obviously should be placed after the end of the general deposition of loess in Central Europe (see section of Gatterleben: Müller 1953, which, of course, does not exclude local deposition of late glacial loesses: see Poser 1951). This leaves little chance for the Hamburgian to be equated with the Magdalenian III or IV of the Périgord (see also Narr 1954: 30 footnote 146). One may ask whether the radiocarbon dates from Magdalenian III layers are really reliable with regard to the nature of the sample and the stratigraphical

position. Moreover, they all come from sites outside the Périgord, and the radiocarbon-dated layers of the Cantabrian Magdalenian III are not necessarily of the same age as those of France; and there, too, retardation and archaic aspects of late sites may play an important and puzzling role especially in more marginal areas, such as Vienne and Indre (Allain and Descouts 1957: 512). This is, of course, even more true when greater distances are involved. Radiocarbon measurements now confirm that the Rhenish Magdalenian of Andernach is later than the general deposition of loess and belongs to the early Allerød (Narr 1952: 3; 1955: 112, 175). This Magdalenian of Andernach, and probably most of the Central European Magdalenian, shows features characteristic of the Magdalenian VIa (Narr 1952). The supposition that in Central Europe a Magdalenian of VIa type still existed when in France the Magdalenian VIb was already fully developed (Narr 1952; 1954; 1955) is also confirmed by radiocarbon dates; and Movius' rather early dating of the French Magdalenian VIb strengthens such a retardation of the German Magdalenian.

Probably other typological complexes, which are numerated according to local stratigraphies in the "Classic Palaeolithic province" of southwestern France, also show some traditions existing side by side. This can already be deduced from the impossibility that the sequence of Proto-Magdalenian, Aurignacian V, Lower, Middle, and Upper Solutrean, Magdalenian I, II, III, IV, V, and VI, is a "unilinear" evolutionary one—let alone the Aurignacian and "Périgordian." Also, changing "Périgordian III" to "Périgordian VI" further widens the gap between the lower and upper "Périgordian" and makes the hypothesis of a Western European "Périgordian" development even more improbable than hitherto supposed; but it favors the concept and terminology of D.A.E. Garrod, whose general lines are now accepted by most Central European archaeologists, especially because radiocarbon datings of the "Périgordian IV" place this later than the Gravettian horizon of Pavlov/Dolní Věstonice, let alone the older Gravettian layers of Willendorf II/5-7. A certain co-existence of the latest "Périgordian" and the Solutrean, and of the latter and the Early Magdalenian, as well as a chronological overlap of certain Early and Middle Magdalenian "stages," which, moreover, show differences in geographical diffusion (Breuil 1937: 44; Menghin 1931: 153; Narr 1954: 6), may help to

diminish some difficulties. One may object, however, that wherever the above mentioned complexes are found in stratigraphic superposition, it is in the indicated order. But that is no less true for the "Lower Périgordian" and the Aurignacian; and yet a certain overlap must be supposed. But this is a rather difficult problem, and one cannot help feeling that some archaeologists are not ready to abandon the predilection for a more or less strict "one after another" sequence. [Göttingen, 28.9.60.]

By KAREL VALOCH

Movius' article, and the comment by Vojen Ložek and Jiří Kukla, may be supplemented by a report on recent investigations of the loess of the Brno region and cave profiles in the Moravian Karst, as well as by the radiocarbon dates De Vries obtained from this material.

Our investigations which were made in the open showed that under the complex of the Würm sediments lie soils of the Riss/Würm (R/W) Interglacial, which are tied in with the 10 m (Riss) terrace of the Svitava River. In quite a number of profiles the R/W soils, which are often doubled and tripled, are divided by a typical aeolian loess which we have denominated Prae-würm (Musil, Valoch, and Nečasny 1954; Musil and Valoch 1955). This is analogous to the so-called "Pariser" of the travertine profile at Ehringsdorf (Behm-Blancke 1960) and to the *cailloutis de solifluction* of the loess of northern France (Bordes 1954). The character of these interglacial soils is indicated by a vertebrate fauna very similar to the total fauna of the lower travertine at Ehringsdorf. Analysis shows that it can be classified with R/W (Musil 1960), and conspicuously differs from the fauna obtained by the Moravian Museum's systematic excavations in the M/R sites of "Stranska skala," near Brno, and the Mladeč Cave in northern Moravia (not published).

The thick loess W I (Y.L. I) is often divided by a loamy zone which indicates a climatic oscillation (W Ia-W Ib), as Movius notes. In a number of investigations, we have been able to ascertain that under favourable conditions two—and in best conditions, three—similar zones are formed. It seems, therefore, that there have been other weak climatic oscillations during the course of the W I.

Above the 5 m terrace of the Svitava River (age W I) at Brno-Malomerice we found a hearth with a few atypical flakes washed away from the former bank, whose age was, consequently, also W I (Musil and Valoch 1961). The radiocarbon test of the charcoal gave

the following result:

BRNO-MALOMERICE
GRO-1482
more than 53,000 years (51,060 B.C.)
(unpublished letter of
Prof. Dr. Hl. De Vries, Aug. 29, 1958).

As a rule, loess W II (Y.L. II) is rather weak and usually is not divided—in contrast to W III (Y.L. III) where, according to Klima (1958) "microinterstadials" are noticeable. The beginning of the W II stadial is characterized by an intense solifluction which in some places formed strong accumulations of floated clays (usually transported W I/II soils). Sometimes the entire section of this epoch is formed by solifluctional sediments. In such profiles, one sort of loess (Y.L. II) is absent, and the Würm complex is formed only by two typical sorts of loess: Y.L. I and Y.L. III.

We were also able to demonstrate the Würm loess complex analogy in the caves of the Moravian Karst. When exploring the "Pod hradem" Cave, we detected a Würm profile, demonstrated both archaeologically and paleontologically, which was divided by the typical positions W I/II and W II/III; they also could be dated with the help of C-14:

POD HRADEM
W I/II upper part.....
GRO-848 charcoal: 32,990 ± 1,500 yrs.
(31,040 B.C.)
GRO-1724 humus: 32,420 ± 470 yrs.
(30,470 B.C.)
W II/III basal zone.....
GRO-1918 humus: 26,240 ± 300 yrs.
(24,290 B.C.)
(unpublished letter of
Prof. Dr. Hl. De Vries, Mar. 26, 1959).

These data are well supplemented by the figures given by Movius for W I/II and W II/III. We regret not having a C-14 date for the Magdalenian in our region, but its stratigraphic position makes certain conclusions possible. Magdalenian implements appear in our caves in the upper part of loess sediments equivalent to Y.L. III; and its further phases of development can be observed in 2, or even 3, soils of different colors overlying it. Earlier Magdalenian (French Magdalenian I-III) is unknown in Czechoslovakia; our earliest Magdalenian industries can be compared to Stage IV of France which, according to Movius, falls into the Daniglacial. From this it may be concluded that in Czechoslovakia the sedimentation of the youngest loess (Y.L. III) continued until the time of the Oldest Dryas; the upper-position soils then must have been formed during the succeeding oscillations. Reliable radiocarbon dates for our Magdalenian and Mesolithic could obviously be a valu-

able contribution to the study of problems of the Late Würm.

[Brno, 15.10.60]

Reply

By HALLAM L. MOVIUS, JR.

A year ago the present writer published a review article in CA (Movius 1960a), which seems to have stirred up considerable interest, especially in the subject of the Würm (Fourth) Glacial succession in Central and Western Europe. Indeed, a whole new set of comments has been received by the Editor, who has requested a "reply." This is a difficult assignment. At the outset, I must admit that, after studying the various contributions, I cannot help experiencing a certain sense of confusion. My own fundamental conviction about the whole problem of the late Upper Pleistocene sequence in Europe is that until the field evidence is intensively examined, assessed, and accepted by my geological colleagues, anything in the nature of a formal "reply" on my part would be not only premature but also out of place. As a Palaeolithic archaeologist, I simply do not feel competent to judge the detailed results of the work of my colleagues in Pleistocene geology, any more than I would expect the latter to enter the archaeological field and contribute observations on such problems, for instance, as the relationship between the so-called "Eastern Gravettian" of Central Europe and the Upper Périgordian of Western Europe. For this reason I feel compelled to rely on what I consider to be the most recent and authoritative syntheses of the geological field evidence. I simply lack the necessary competence to judge the validity of the latter (indeed I have never even seen most of the localities in question), and therefore I am not willing to enter into discussions relative to the significance of this or that loam horizon, weathering surface, etc., exposed in a given section. These are geological problems and they have no place in the realm of my own sphere of professional training and interest, *except* insofar as they bear on the problem of Palaeolithic archaeology. When geologists whose views I respect and in whose judgment I have confidence have synthesized the data and produced a sequence that is considered reasonably reliable and complete, then I shall modify my own views accordingly. Until that time I shall continue to follow the conclusions of those scientists, the results of whose correlation studies are based on the most reliable and up-to-date evidence. In other words, the pres-

ent writer does not intend to put himself in the position of sitting in judgment over controversies that lie entirely within the confines of a discipline, which, although it bears directly on his own field, concerns the interpretation of data for which his own background and training do not qualify him as a specialist.

Several commentators (e.g. Kozłowski and Narr) refer to the present status of the Périgordian problem, pointing out that serious doubts concerning the correctness of D. Peyrony's scheme have arisen as the result of recent intensive research in the Dordogne region. Peyrony originally claimed that the Upper Périgordian of southwestern France was derived from the Lower Périgordian, or Châtelperronian, a concept which in the present writer's opinion still has considerable merit. The trouble is that the arguments set forth by Peyrony in support of his claim are no longer valid. For, in reality, his Périgordian II belongs to the Aurignacian (Movius 1960a: 368 fn. 8, 386), while his Périgordian III comes at the end of the sequence and is being called Périgordian VI for the present (Movius 1960b: 385; Movius and Vallois 1960: 221). Thus, one is left with nothing between the I and IV stages of Peyrony's original sequence—a long gap during which the entire Aurignacian I-IV developments took place in Western Europe. But there is new evidence from two sites in the Department of Vienne—Les Cottés and Fontenioux—which will be summarized below, indicating that some sort of a "Middle" Périgordian development did in fact take place. Although both sites have been published, their significance and important bearing on the Upper Palaeolithic succession in Western Europe do not seem to be widely known.

Les Cottés—Here Pradel (1959) found a Périgordian level underlying the classic Aurignacian I occupation and separated from it by a layer of *éboulis*. The assemblage from this lower level includes unquestionable Périgordian I (Châtelperronian) types, on the one hand, associated with what one might call "Proto-Gravettian" (i.e. Périgordian IV) forms, on the other. Pradel has proposed the term Périgordian II for this assemblage. Certainly there is no denying the fact that it is an "evolved" sort of Périgordian I which includes many artifacts that could perfectly well have been found in *Couche 5* (Périgordian IV) at the Abri Pataud, Les Eyzies (Dordogne), where the writer is currently excavating.

Fontenioux—This site, also excavated by Pradel (1952, 1953; de Sonneville-Bordes 1960: 132-35, 176-77), contained two occupation levels, the upper one of which yielded an "evolved" Aurignacian assemblage considered by Pradel and Mme. Bordes to be reminiscent of Peyrony's Aurignacian V at Laugerie-Haute. From a

typological point of view, the materials from the underlying horizon are midway between the assemblage from the lower level at Les Cottés and the true Périgordian IV of La Gravette and the Abri Pataud. Admittedly this is rather slender evidence, but since Pradel has a fairly complete sequence of Périgordian V from the nearby site of Laroux, these data seem to support the view that the assemblage from the lower horizon at Fontenioux does indeed belong to some sort of "middle" stage of the Périgordian that is already very reminiscent of Périgordian IV.

In connection with Peyrony's hypothesis, one must also mention the evidence of Laugerie-Haute, where the Aurignacian V occurs in between the underlying Proto-Magdalenian and the overlying Proto-Solutrean. Here there is good stratigraphic evidence demonstrating the late persistence and reappearance in the Périgord of the Aurignacian into post-Upper Périgordian times.

Samples from Les Cottés have been sent to the Groningen C-14 laboratory for measurement; unfortunately, none was collected at Fontenioux at the time of the excavation, and the deposits have been completely removed. Therefore it is impossible to obtain a radiocarbon date for this site.

Although the facts set forth above are by no means conclusive, Professor and Mme. Bordes agree with the present writer with respect to their significance. In any event, it is difficult to deny that a very strong case can be made in support of the validity of at least part of Peyrony's original hypothesis concerning an evolutive liaison between the Lower and Upper Périgordian developments in southwestern France.

On comments of Gross: More than any other single worker, Gross has published a series of very significant and important papers bearing on the problem of the chronology of the Last Glaciation. The present writer is particularly grateful to him for his very helpful advice and generous assistance in compiling a radiocarbon-dated chronology for the Würm Glaciation to serve as a basis for interpreting the classic Upper Palaeolithic cultures of southwestern France in terms of this sequence and in the light of the available C-14 dates. His very painstaking critical review of the present status of the whole Würm problem constitutes a major and very important contribution. Certainly the present writer feels his conclusions are the most plausible and logical that can be formulated on the basis of the existing evidence. For this reason, one is inclined to accept his view concerning the dating of the Göttweig Interstadial Complex—i.e., that

it began ca. 48,000 or 47,000 B.P. and persisted until ca. 32,000 B.P. Apparently it was an exceedingly long period of unstable climate when conditions oscillated between arctic/subarctic and cool temperate intervals of varying duration. On the basis of the present data, it is impossible to be more precise. The evidence supporting this dating, together with his critical assessment of various alternative proposals, have been clearly organized and set forth by Gross in his pertinent and very timely comments.

On comments of Kozłowski: The information that the Russian nuclear physicist, S. A. Jakovlev, has developed new dating techniques based on either (a) radium and actinium, or (b) radium and uranium, is of great interest and potential importance. One would like to know more about these methods. Samples from Kostienki: layer 5 measured by radium/actinium (39,000 B.P.) and by radium/uranium (27,000 B.P.) reveal a discrepancy of 12,000 years; from Kostienki I: upper stratum the results show a discrepancy of 5,000 years in the opposite direction, since the radium/actinium figure is 23,000 B.P., while that for radium/uranium is 28,000 B.P. The probable error is not stated in either instance. Independent measurements by the radioactive carbon method would perhaps throw light on this subject.

Few Palaeolithic archaeologists in Western Europe are aware of the fact that "lamelles Dufour" occur as an important component of certain Upper Palaeolithic assemblages in Central and Eastern Europe attributed to the Aurignacian on the basis of typologic evidence. Hence the information set forth in Kozłowski's comment concerning the known distribution of these tools in Poland, western Rumania, and the Crimea, as well as Austria, is of the greatest interest. As in Western Europe, where "lamelles Dufour" also occur in an Aurignacian context, these artifacts are not typical of any given stage.

Kozłowski stresses the importance of obtaining absolute dates as a means of contributing to a clearer understanding of the Szeletian problem. Manifestly the evidence which he cites—i.e., the occurrence of a single side-scraper at the Salzofen Cave (C-14 date—GRO-761: 34,000 B.P. \pm 3,000 yrs.) which has typological analogies with the Tata industry of Hungary—can scarcely be accepted as demonstrating the antiquity of the Central European leaf-point industries.

It should be pointed out that the C-14 dates for the Périgordian IV (*Couche 5*) horizon at the Abri Pataud

(W-151: 23,600 B.P. \pm 800 yrs.; W-152: 24,000 B.P. \pm 1,000 yrs.) are very likely too young in view of the 1959 results obtained by the Groningen laboratory for the overlying Proto-Magdalenian (*Couche* 2) horizon (ca. 20,250 B.P.). For intervening between the two occupations, two extensive levels—Périgordian VI (*Couche* 3) and Périgordian Vc (*Couche* 4)—occur. It is felt that when new samples from *Couche* 5 have been processed and measured by the more refined and accurate techniques developed during the past five years, the resulting figure for the Périgordian IV horizon (up to 70 cm. thick in places) will very likely be of the order of 25,000 B.P. \pm 1,000 yrs.

Kozłowski has apparently misunderstood the stratigraphic position of the Périgordian VI *couche* at the Abri Pataud, as well as at the nearby site of Laugerie-Haute: Est, Les Eyzies (Dordogne). At both of these large rock-shelters, this occupation definitely underlies extensive Proto-Magdalenian levels. It is the latter rather than the Périgordian VI which is believed to be approximately the same age as the maximum of the Brandenburg Stage in northern Europe, on the basis of the palaeontological, geological, and radiocarbon evidence. In this connection, the present writer, who has not had an opportunity of examining the alleged Périgordian VI assemblage from Mafy Antoniów (District of Starachowice), is unable to comment on Kozłowski's statement concerning the existence in Poland of the Périgordian VI Stage of Western Europe.

On comments of DE HEINZELIN: De Heinzelin's contribution makes possible a tentative geographic extension into the Strasbourg region and Belgium of the chronological scheme set forth elsewhere by the present writer (Mövius 1960a); consequently it is of great importance. One statement of fact concerning the locality of Goderville, near Le Havre in northern France, calls for comment. De Heinzelin is correct in his claim that this loess station is "practically the only [open-air] site which has yielded the Lower Périgordian in a stratified context." Therefore, any attempt "to equate the loess deposits with those of the caves must hinge on this point, which has not been made entirely explicit." But Bordes' attribution of the overlying loess to the Young Loess III of the Paris Basin is correct, which means that the horizon in question is of Göttweig age. According to Bordes' scheme, both Young Loess I and II are of Early Würm age (Mövius 1960a: 365). On this basis, de Heinzelin's suggestion that "the Périgordian I actually appeared at the base of the Y.L. II" is somewhat misleading, notwith-

standing the fact that, if such were indeed the case, "many contradictions would disappear, quite as much with respect to correlations with the cave deposits as with respect to correlations with Central Europe."

In the Dordogne region of southwestern France the Upper Périgordian horizons are not associated with thermoclastic sediments, as is the case with the Proto-Magdalenian and the Aurignacian V. On the contrary, there is evidence indicating that the Upper Périgordian in the Dordogne is associated with a relatively cool temperate climate, apparently less rigorous than that which prevailed during Aurignacian II times. However, as yet it would be premature to speculate concerning the position of the Paudorf Oscillation (de Heinzelin's Interstadial II/III) in terms of the archaeological succession. Certainly de Heinzelin's suggestion that the Middle Périgordian (if indeed such a development does exist) belongs in this interval should be treated with all possible reservation.

For the relatively mild interval during which the Solutrean and the Lower Magdalenian developments occurred in southwestern France, de Heinzelin proposes the term Interstadial III/IV, which corresponds to Müller-Beck's "Dordogne Phase" (see below). Inasmuch as the present writer is unequivocally opposed to any system for designating subdivisions that is based on the use of numbers (which only leads to confusion and misunderstandings), it is his conviction that this proposal has little to commend it. For this reason it is felt that Müller-Beck's term is preferable.

Finally, de Heinzelin registers a plea for a more precise terminology with respect to designating the various subdivisions included within the Würm Glaciation. In this connection, it is difficult to understand why he feels that "most of these terms have an internationally codified stratigraphic significance." Certainly this is true with reference to rock units in historical geology. Unfortunately, notwithstanding the suggestions of de Heinzelin and Müller-Beck, the problem of an acceptable terminology for the various Upper Pleistocene events under discussion is still far from solution.

On comments of BRANDTNER: Brandtner's clear explanation of the exact stratigraphic position of the much-discussed charcoal sample from Senftenberg, near Krems in Lower Austria, is of great importance with reference to establishing the age of the Göttweig Interstadial Complex. One will await with interest the publication of his monograph on this site. In the meantime, his statement that in his opinion,

"the charcoal cannot provide any date for the interstadial [i. e., Göttweig] weathering process, but only for the prior deposition of the loess" contributes substantially to a better understanding of the significance of the late Prof. De Vries' C-14 date for this important locality.

On comments of VALOCH: Valoch's brief contribution includes a summary of his own, Musil's, and Nečesaný's conclusions concerning the often doubled and even tripled fossil soil complex underlying extensive Würm sediments in the vicinity of Brno (Moravia). Certain authorities consider these soils to be in part at least of Mindel/Riss Interglacial age, but Valoch places them in the Riss/Würm Interglacial on the basis of the fact that they are tied in with the 10-meter (Riss) terrace of the Svitava River. The present writer, who has never studied the sections in question, is unable to comment on the correctness of this interpretation, which seems to be substantiated by the paleontological evidence.

The results of radiocarbon measurements determined by the late Prof. De Vries for samples collected by Valoch *et al.* in the well-stratified caves of the Moravian Karst, and published herein for the first time, are of more than passing interest. Thus the figures for the upper part of the horizon at "Pod hradem" Cave attributed to the Göttweig Interstadial Complex (GRO-848 and GRO-1724) and the basal zone of the Paudorf deposits at the same locality (GRO-1918) are in excellent agreement with the chronology advocated by Gross (see comments above) and the present writer (Mövius, 1960a).

On comments of KUKLA and KLÍMA: On the basis of the results of recent radiocarbon measurements of samples collected at the classic site of Dolní Věstonice, near Mikulov in southern Moravia, our Czech colleagues Kukla, Klíma, (and Ložek) suggest that the formation of the Göttweig Chernosem soils began prior to 52,000 B.P. Indeed, they even suggest the possibility that the formation of the Göttweig Interstadial Complex also includes the Brörup-Loopstedt Interstadial (including Amersfoort XII and the Mussolini Canal log). But this suggestion is apparently contradicted by the new radiocarbon date for sample 3 from the classic exposure of Göttweig fossil soils in the Oberfellabrunn brickyard, near Hollabrunn in Austria.¹ Accordingly, the present writer is more than skeptical of the new Dolní Věstonice figure. Certainly before basing a chronology on the evidence, many additional control measurements are

¹Unfortunately, Prof. De Vries died before completing his final analysis of the Oberfellabrunn samples.

urgently needed, not only from other localities in Czechoslovakia, Austria, and adjacent regions of Central Europe, but also from Western Europe as well.

The age of the Paudorf Oscillation (or ? Interstadial) remains somewhat obscure. Kukla and Klima state that "it seems likely that the Paudorf interval was a relatively long, wet, mild or warm period lasting at least 10,000 years," but do not cite the evidence on which this statement is based. The present writer knows of no figures to support the contention that "many radiocarbon dates from the period between 22,000 and 31,000 years ago show evidence of a comparatively mild climate." However, one must admit that the Paudorf Oscillation probably covered a somewhat longer interval than two millenia, as originally suggested on the basis of figures available in March, 1960 (compare Movius 1960a: 262). Two new dates for the strongly decalcified soil underlying the Eastern Gravettian occupation layer at Dolní Věstonice demonstrate that the lower limits of this interval must be placed earlier than had previously been indicated. These two dates, which are referred to by Kukla and Klima in their comment, are as follows:

GRO-2092 28,100 ± 380 yrs.
(26,150 B.C.)

GRO-2598 28,900 ± 300 yrs.
(26,950 B.C.)

A sample of charcoal from the cultural horizon at this extensive site, which is situated in the uppermost portion of the Paudorf soil horizon, gave the following figure (Movius 1960a: 362):

GRO-1286 25,600 ± 170 yrs.
(23,600 B.C.)

The new date for the "Pod hradem" Cave in the Moravian Karst region, listed by Valoch in his comment, is in good agreement with this figure; it is as follows:

GRO-1918 26,240 ± 300 yrs.
(24,290 B.C.)

The date obtained for the nearby locality of Pavlov (Pollau), where the occupation layer is situated immediately above the Paudorf fossil soil (Movius, 1960a: 362), supports the view that this oscillation had already ended by about 23,000 B.C. The Pavlov date is given below:

GRO-1325 24,800 ± 150 yrs.
(22,850 B.C.)

On the basis of the present evidence, therefore, one concludes that this oscillation (or ? Interstadial) was at least 4,000 years long, although admittedly no dates marking its beginning are yet available.

On Comments of MÜLLER-BECK: Müller-Beck's contribution serves to emphasize the fact that the whole prob-

lem of Upper Pleistocene nomenclature—i.e., the terminology to be used with reference to clearly recognized and widespread stages, substages, etc., that can be used as points of reference in establishing regional chronologies—is in urgent need of careful revision. The thesis which he has propounded is one possible solution to this dilemma, and accordingly it should be carefully considered. However, the present writer does not feel competent to pass judgment. For this whole problem falls in the field of the natural sciences: it should be decided by Pleistocene geologists and *not* by Palaeolithic archaeologists. For this reason the present writer's "reply" to Müller-Beck's contribution will be limited to the following brief comments:

a) Introducing the term "Late Interglacial Interstadial Complex" to include events (i.e., the Brøup-Loopstedt Interstadial) that are demonstrably of Early Würm age seems confusing.

b) The Lebenstedt Phase as marking an interval when cold conditions prevailed definitely has validity, but Müller-Beck's reasons for considering that this marks the beginning of an "Early Glacial Phase," which, together with the preceding "Late Interglacial Interstadial Complex," constitutes a main "Anaglacial Phase," are difficult to understand.

c) Only with the next onset of cold conditions, which is referred to as the Vogelherd-Achenheim Phase, does Müller-Beck begin his "Full Glacial"—i.e., Lower Stage of the Würm Glaciation, according to Müller-Beck's scheme. This corresponds with what the present writer has referred to as the Early Phase of the Main Würm. However, the present writer, who recently visited the Vogelherd Cave and examined the archaeological materials found there by Riek (1934; 1960: 71-87), can find no a priori grounds for assigning either of the two Aurignacian occupations of this important locality to such an early horizon.

d) The Paudorf Interstadial (or ? Oscillation) is considered to separate the Vogelherd-Achenheim Phase, or "Lower Stage of the Full Glacial," from the Pollau (Pavlov) Phase. The following mild interval, for which Müller-Beck proposes the term Dordogne Phase (= de Heinzelin's Interstadial III/IV), was brought to a close by the onset of cold conditions (= Oldest Dryas: paleobotanical Zone Ia), the entire series of events—i.e., from the beginning of the Paudorf interval to the end of Oldest Dryas times constituting the "Upper Stage of the Full Glacial."

e) Finally, the Late Glacial Phase in Müller-Beck's sequence corresponds in the main with what the present writer has referred to as the Late Würm Stage.

In essence, if the present writer understands Müller-Beck's scheme correctly, he proposes (a) that the entire span of time since the Riss-Würm Interglacial covered by the Early Würm (including its several oscillations) and the Göttweig Interstadial Complex should be grouped in a single main stage, the *Anaglacial*; and (b) that all events of Main Würm (including the

Paudorf Oscillation) and Late Würm age should be assigned to a second main stage, the *Würm Glacial*. Notwithstanding the fact that, from a terminological point of view, the scheme set forth elsewhere by the present writer (Movius 1960a) is in many respects unwieldy, he nonetheless feels that in the light of our present knowledge it lends itself to greater precision than does the one proposed by Müller-Beck. Certainly the latter's contribution emphasizes the urgent need for the establishment of regional sequences and for a terminology to designate the *cold maxima* within the Würm. Accordingly, certain of his proposals should be given careful consideration. In other words, it is particularly to the terminology proposed by Müller-Beck for designating the main phases of the Last Glaciation (A and B on his chart) that the present writer objects.

On comments of NARR: Because of terminological difficulties—for example, use of the term "Early Glacial Period" for designating a span of time manifestly later than the Early Würm—it is exceedingly difficult to follow the strictly geological section of Narr's comments. Certainly if he had presented a substantive argument for an early dating of the Göttweig Interstadial Complex which he suggests ended "around, or more probably before, 50,000 B.C.," then the present writer would be inclined to agree that this possibility "should at least be borne in mind as an alternative solution." Granting the supporting evidence is clear in Narr's mind, however, he has failed to organize and present it in a convincing manner. Since the basis for Narr's geological sequence is obscure, the present writer proposes to confine his "reply" to a brief consideration of certain of the archaeological aspects of this contribution.

Very few Western archaeologists have ever seen Vértes' so-called "Aurignacian II" series from Istállóskő Cave, situated in the Bükk Mountains (Hungary). Certainly neither the text nor the illustrated pieces in Vértes' report (Vértes *et al.* 1955) supports the claim that this assemblage "has traits suggesting that it was in contact with the oldest horizon of Gravettian elements in Central Europe."

The stratigraphic facts established by Bordes for the Young Loess sequence in the Paris Basin are not in accord with the correlations which Narr has proposed. Therefore there is no basis for the claim that the "Série mate" of Goderville is approximately the same age as the culture layers of Pavlov and Dolní Věstonice. For further discus-

sion of the Goderville locality, see Movius (1960a: 365) and the "reply" to de Heinzelin's comments in the present issue of CA.

The present writer, who has spent the past two field seasons excavating an extensive Périgordian Vc occupation layer at the Abri Pataud, Les Eyzies (Dordogne), would be interested to know why this level "should be placed later than Paudorf-Stillfried B." In point of fact, no deposits that can definitely be assigned to the Paudorf have ever been described in southwestern France.

One fails to understand the basis for Narr's objection to the present writer's contention that the Hamburgian of northern Germany and the Middle Magdalenian (III-IV) of southwestern France are of approximately the same age. Certainly this is suggested not only by the geological evidence, but also by the radiocarbon dates. Admittedly Narr may be quite correct in pointing out that "the radiocarbon-dated layers of the Cantabrian Magdalenian III are not necessarily of the same age as those of France." However, to evoke "retardation and archaic aspects of late sites" in support of this contention seems inadmissible speculation, especially since there is absolutely no evidence available at present to support such a claim.

Further comments on Narr's conception of the Upper Palaeolithic sequence in southwestern France seem unnecessary. Suffice it to add that certain of the problems which he has set forth are at present under intensive investigation. At this juncture various working hypotheses are beginning to emerge, and these will be fully discussed in an article now in preparation. In the meantime, as stated elsewhere (Movius 1960a: 387), it does not seem likely that pure unfounded speculation, which has no bearing whatsoever on the field evidence, is likely to advance our knowledge of the very complex nature of the Upper Palaeolithic sequence in the classic Dordogne region of southwestern France.

References Cited

ALLAIN, (Dr.) JACQUES, and J. DESCOUTS. 1957. A propos d'une baguette à rainure armée de silex découverte dans le Magdalénien de Saint-Marcel. *L'Anthropologie* 61, No. 5-6:503-12.

ANDERSEN, SVEND TH. 1957. New investigations of interglacial fresh-water deposits in Jutland. A preliminary report. *Eiszeitalter und Gegenwart* 8:181-86.

ANDERSEN, SV. TH., HL. DE VRIES, and W. H. ZAGWIJN. 1960. Climatic change and radiocarbon dating in the Weichselian

Glacial of Denmark and the Netherlands. *Geologie en Mijnbouw* 39:38-42.

BAYER, JOSEF. 1927. *Der Mensch im Eiszeitalter*. I und II Teil. Leipzig and Wien: F. Deuticke. 452 pp., 220 figs.

BEHM-BLANCKE, G. 1960. Altsteinzeitliche Rastplätze im Travertingebiet von Taubach, Weimar, Ehringsdorf. *Alt Thüringen* 4, 246 pp., 66 figs.

BLANC, A. C. 1960. Comment. *Current Anthropology* 1:375-76.

BLANC, A. C., HL. DE VRIES, and M. FOLLIERI. 1957. A first C14 date for the Würm I chronology on the Italian coast. *Quaternaria* 4:83-93. Roma.

BOHMERS, ASSIEN. 1941a. Reiche Funde eiszeitlicher Bildkunst. Die Ausgrabungen bei Unter-Wisternitz. *Germanien* 13, Hft. 2:45-57.

—. 1941b. Die Ausgrabungen bei Unter-Wisternitz. Neue Ergebnisse der grössten paläolithischen Fundstelle. *Forschungen und Fortschritte* 17, Nr. 3:21-22.

BORDES, FRANÇOIS H. 1947. "Les limons quaternaires du bassin de Paris et du nord de la France," in *La Géologie des Terrains Récents dans l'Ouest de l'Europe* (Session Extraordinaire des Sociétés Belges de Géologie [Bruxelles, 19-26 Septembre 1946]), pp. 362-72.

—. 1952. Stratigraphie du loess et évolution des industries paléolithiques dans l'Ouest du bassin de Paris. *L'Anthropologie* 56, No. 1-2:1-39; 56, No. 5-6:405-52.

—. 1954. Les limons quaternaires du bassin de la Seine. Stratigraphie et archéologie paléolithique. *Archives de l'Institut de Paléontologie Humaine*, Mémoire 26. 472 pp.

—. 1958. Nouvelles fouilles à Laugerie-Haute Est. Premiers résultats. *L'Anthropologie* 62, No. 3-4:205-44.

—. 1959. Laugerie-Haute, coupe de la partie centrale de la fouille F. Bordes. *Gallia Préhistoire* 2:160-61.

—. 1960a. Analyse de la publication de P. Wernert (1957) sur Achenheim. *L'Anthropologie* 64:77-85.

—. 1960b. Comment. *Current Anthropology* 1:376-77.

BORDES, FRANÇOIS H., and HANSJÜRGEN MÜLLER-BECK. 1956. Zur Chronologie der Lössedimente in Nordfrankreich und Süddeutschland. *Germania* 34, Hft. 3/4: 199-208.

BORDES, F. H., and P. FITTE. 1949. Les limons de la région de Villejuif et leurs industries paléolithiques. *L'Anthropologie* 53: 1-19, 193-208, 407-33.

BRANDTNER, FRITZ. 1954. Jungpleistozäner Löss und fossile Böden in Niederösterreich. *Eiszeitalter und Gegenwart* 4/5:49-82.

—. 1956. Lösstratigraphie und paläolithische Kulturabfolge in Niederösterreich und in den angrenzenden Gebieten. (Zugleich ein Beitrag zur Frage der Würmgliederung.) *Eiszeitalter und Gegenwart* 7:127-75.

—. 1959. Die geologisch-stratigraphische Position der Kulturschichten von Willendorf i.d. Wachau, N. Ö. *Mitteilungen der Prähistorischen Kommission der Österreichischen Akademie der Wissenschaften* 8/9:173-98.

BREUIL, (l'Abbé) HENRI. 1937. *Les subdivisions du Paléolithique supérieur et leur signification*. 2nd ed. Lagny: E. Grevin. 78 pp. (See also: *Compte Rendu du Congrès International d'Anthropologie et d'Archéologie Préhistorique*, 14ème Session, Genève, 1912, pp. 165-268; 1st ed.)

BREUIL, (l'Abbé) HENRI, and L. KOSLOWSKI. 1931-1934. Etudes de stratigraphie paléolithique dans le nord de la France, la Belgique et l'Angleterre. *L'Anthropologie* (1931) 41:449-88; (1932) 42:27-47, 291-314; (1934) 44:248-90.

BRODAR, SREČKO. 1938. Das Paläolithikum in Jugoslawien. *Quartär* 1:140-72.

—. 1939. O stratigrafiji Potocke zijalke. (Über die Stratigraphie der Potocka-Höhle.) *Glasnik Muzejskega Društva za Slovenijo* 20, No. 1-4:66-95. (German summary, pp. 91-95.)

BRUNNACKER, KARL. 1953. Der würmezeitliche Loess in Bayern. *Geologica Bavarica* 19:258-65.

—. 1956a. Regionale Bodendifferenzierungen während der Würmezeit. *Eiszeitalter und Gegenwart* 7:43-48.

—. 1956b. Die Höhlensedimente im Hohlen Stein bei Schambach. *Geolog. Blatt NO-Bayern* 6:21-32.

—. 1956c. Das Lössprofil in Kitzingen (Unterfranken): Ein Beitrag zur Chronologie des Paläolithikums. *Germania* 34: 3-11.

—. 1957. Die Geschichte der Böden im jüngeren Pleistozän in Bayern. *Geologica Bavarica* 34:3-95.

—. 1958. Zur Parallelsierung des Jungpleistozäns in den Periglazialgebieten Bayerns und seiner östlichen Nachbarländer. *Geolog. Jahrbuch* 76:129-50.

BÜDEL, JULIUS. 1949. Die räumliche und zeitliche Gliederung des Eiszeitklimas. *Naturwissenschaften* 36:105-12, 133-39.

—. 1950. Die Klimaphasen der Würmezeit. *Die Naturwissenschaften* 37, Hft. 19:438-49.

—. 1951. Die Klimazonen des Eiszeitalters. *Eiszeitalter und Gegenwart* 1:16-26.

—. 1959. Periodische und episodische Solifluktion im Rahmen der klimatischen Solifluktionstypen. *Erdkunde* 13:298-314.

—. 1960. Die Gliederung der Würmkaltzeit. *Würzburger Geographische Arbeiten* 8, 45 pp., 4 figs.

CHMIELEWSKA, M., and W. CHMIELEWSKI. 1960. Stratigraphie et chronologie de la dune de Witów distr. de Łeczycza. *Biuletyn Pręglacjalny* 8.

DELPORTE, HENRI. 1958. Notes de Géographie Préhistorique: I—Les Pointes d'Aurignac. *Annales de la Faculté des Lettres de Toulouse* 7:11-29.

DE HEINZELIN, JAN. 1958. Problèmes du Pleistocènes dans le Middle West (U.S.A.). *Bulletin de la Société Belge de Géologie* 67:265-90.

DE SONNEVILLE-BORDES, DENISE. 1960. *Le Paléolithique supérieur en Périgord*. Bordeaux: Delmas. 2 tomes. 558 pp., 295 figs., 64 tables.

DE SONNEVILLE-BORDES, DENISE, and FRANÇOIS H. BORDES. 1958. Position stratigraphique de l'Aurignacien V à Laugerie-Haute Est. *L'Anthropologie* 62, No. 3-4:378.

DE VRIES, HESSEL. 1958. Radiocarbon dates for upper Eem and Würm interstadial samples. *Eiszeitalter und Gegenwart* 9: 10-17.

—. 1959. Radiocarbon dating of the fossil soils at Ober Fellabrunn. *Proceedings of the Koninklijke Akademie van Wetenschappen*, ser. B, 62, No. 1:84-91. Amsterdam.

DE VRIES, HESSEL, and H. T. WATERBOLK. 1958. Groningen radiocarbon dates III. *Science* 128, No. 3338:1550-56.

EBERS, EDITH. 1960. Drumlinkerne und das Würm-Interstadialprofil von Hörnating

- in Oberbayern. *Eiszeitalter und Gegenwart* 11. (In press.)
- EHRENBERG, KURT. 1953. Die paläontologische prähistorische und paläoethnologische Bedeutung der Salzofenhöhle in Lichte der letzten Forschungen. *Quartär* 6, Halbbd. 1:19-58.
- EMILIANI, CESARE. 1955. Pleistocene temperatures. *The Journal of Geology* 63:538-78.
- . 1958. Paleotemperature analysis of core 280 and Pleistocene correlations. *The Journal of Geology* 66:264-75.
- FELGENHAUER, F. 1959a. Das Paläolithikum von Willendorf in der Wachau, Niederösterreich. Vorbericht über die monographische Bearbeitung. *Forschungen und Fortschritte* 33, No. 3:152-55.
- . 1959b. Willendorf in der Wachau. Monographie der Paläolith-Fundstellen I-VII. Mit Beiträgen von F. Brandtner, A. Papp, E. Thenius und E. Zirkl. *Mitteilungen der Prähistorische Kommission der Akademie der Wissenschaften* 8/9, Teile 1-3 (1956-1959), 296 pp. and 124 Abb. Wien.
- FELGENHAUER, FRITZ, JULIUS FINK, and HESSEL DE VRIES. 1959. Studien zur absoluten und relativen Chronologie der fossilen Böden in Österreich. *Archaeologia Austriaca* 25:35-73.
- FINK, J. 1956a. Zur Systematik fossiler und rezenter Lössböden in Österreich. *11^e Congrès International de la Science du Sol, Paris*: 585-92.
- . 1956b. Zur Korrelation der Terrassen und Löss in Österreich. *Eiszeitalter und Gegenwart* 7:49-77.
- FRISINA, H. 1951. Neue Ergebnisse der Lössforschung im nördlichen Württemberg. *Jahreshefte d. Geolog. Abteilung d. Württembergischen Statistischen Landesamtes* 1:54-59.
- FRUND, G. 1952. Die Blattspitzen des Paläolithikums in Europa. 349 pp., 16 figs.
- FRYE, JOHN C., and H. B. WILLMAN. 1960. *Classification of the Wisconsinian Stage in the Lake Michigan Glacial Lobe*. Illinois State Geological Survey, Circular 285. Urbana.
- GARROD, D. A. E., et al. 1939. Excavations in the Cave of Bacho Kiro, North-East Bulgaria. *Bulletin of the American School of Prehistoric Research* 15:46-84.
- GORODEV, V. A. 1940. Rezultaty issledovaniia ilskoi paleoliticheskoj stoinki. *Materialy i issledovaniia po arkeologii SSSR* 59.
- GÖTZINGER, G. 1935. Zur Gliederung des Lösses. Leimen und Humuszonen im Viertel unter dem Manhartsberge. *Verhandlungen der Geologischen Bundesanstalt* 8/9:126-32.
- . 1936. Das Lössgebiet um Göttweig und Krems an der Donau. *Führer für die Quartär-Exkursionen in Österreich*, I: 1-11.
- GRAU, H. 1951. Problemas de la Estratigrafía Pleistocena en el Antepais Alpino Alemán. *Geographia Una et Varia*:119-31. Tucuman: Universidad Nacional, Instituto de Estudios Geográficos.
- . 1952. Zur Gliederung der mittelpleistozänen Ablagerungen in Oberschwaben. *Eiszeitalter und Gegenwart* 2:133-46.
- . 1953. Bemerkungen zur Würmstratigraphie im Alpenvorland. *Geologica Bavaria* 14:124-39.
- . 1955. Bemerkungen zu einer geologischen Übersichtskarte des Iller-Riss-Gebietes. *Zeitschrift der deutschen Geol. Ges.* 105 (1953):517-24.
- GROSS, HUGO. 1951. Die moorgeologische Datierung des jüngeren Magdalénien in Deutschland. *Eiszeitalter und Gegenwart* 1:166-71.
- . 1954. Des Alleröd-Interstadial als Leithorizont der letzten Vereisung in Europe und Amerika. *Eiszeitalter und Gegenwart* 4/5:189-209.
- . 1955. Weitere Beiträge zur Kenntnis des Spätglazials. *Eiszeitalter und Gegenwart* 6:110-15.
- . 1956. Das Göttweiger Interstadial, ein zweiter Leithorizont der letzten Vereisung. *Eiszeitalter und Gegenwart* 7: 87-101.
- . 1957a. Die Fortschritte der Radiokarbonmethode, 1952-1956. *Eiszeitalter und Gegenwart* 8:111-80.
- . 1957b. Die geologische Gliederung und Chronologie des Jungpleistozäns in Mitteleuropa und den angrenzenden Gebieten. *Quartär* 9:3-39.
- . 1958. Die bisherigen Ergebnisse von C¹⁴-Messungen und paläontologischen Untersuchungen für die Gliederung und Chronologie des Jungpleistozäns in Mitteleuropa und den Nachbargebieten. *Eiszeitalter und Gegenwart* 9:155-87.
- . 1959. Noch einmal: Riss oder Würm? *Eiszeitalter und Gegenwart* 10: 65-76.
- . 1960a. Comment. *Current Anthropology* 1:377-80.
- . 1960b. Die Bedeutung des Göttweiger Interstadials im Ablauf der Würm-Eiszeit. *Eiszeitalter und Gegenwart* 11: 99-106.
- HANTKE, R. 1958. Die Gletscherstände des Reuss- und Linthsystems zur ausgehenden Würmeiszeit. *Eclogae Geol. Helv.* 51: 119-49.
- HARING, A., A. E. DE VRIES, and HESSEL DE VRIES. 1958. Radiocarbon dating up to 70,000 years by isotopic enrichment. *Science* 128, No. 3322:472-73.
- HIJZELER, C.C.W.J. 1957. Late-Glacial human cultures in the Netherlands. *Geologie en Mijnbouw, n.s.*, 19:288-302.
- International subcommission on stratigraphic terminology*. International Geological Congress.
- Jahrbuch Bern. Hist. Mus.* 1959. Ur- und Frühgeschichtliche Fundstatistik, Mesolithikum: Seeberg (Bezirk Wangen a.d.A.). *Jahrb. Bern. Hist. Mus.* 37/38 (1957-58):170-71.
- JAKOVLEV, S. A. 1955. Metod opredelenia vozrasta po sotnocheniiu radioaktivnikh elementov v kostiakh. *Metodicheskoe rukovodstvo po geologicheskoi snimke teletvritel'nykh otzjemi*. Vol. 1. Moscow.
- KAHKE, H. D. 1958. Die jungpleistozänen Säugetierfaunen aus dem Travertengebiet von Taubach-Weimar-Ehringsdorf (Vorbericht). *All-Thüringen* 3:97-130.
- KLIMA, B. 1958. Výzkum Paleolitického Sídliště u Pavlova. (Fouilles de la station paléolithique de Pavlov en 1956.) *Archeologické Rozhledy* 10, No. 1:3-15. (French summary, pp. 14-15.)
- KNOR, A., V. LOŽEK, J. PELIŠEK, and K. ŽEBERA. 1953. Dolní Věstonice. Výzkum Táborského Lovců Mamutů v letech 1945-1947. *Československá Akademie Věd, Monumenta Archeologica*, 2, 87 pp., 13 tables. Praha. (French summary at end of each chapter.)
- KRUKOWSKI, S. 1939. *Paleolit. Prehistoria ziem polskich*. Kraków.
- LAIS, ROBERT. 1941. Über Höhlensedimente. *Quartär* 3:56-108.
- . 1951. Über den jüngeren Löss in Niederösterreich, Mähren und Böhmen. *Berichte der Naturforschende Gesellschaft zu Freiburg-im-Breisgau* 41, Hft. 2:119-78.
- . 1954. Über den Löss von Unterwisternitz (Mähren). *Palaeohistoria* 2:135-73.
- LEONARD, A. BYRON, and J. C. FRYE. 1960. Wisconsinian molluscan faunas of the Illinois valley region. Illinois State Geological Survey, 304.
- LEROI-GOURHAN, ANDRÉ. 1959. Résultats de l'analyse pollinique de la grotte d'Isturitz. *Bulletin de la Société Préhistorique française* 56:619-24.
- LOŽEK, VOJEN. 1955. *Měkkýši československého Kvartéru*. (Molluscs of the Tschecoslovakischen Quartärs.) *Rozprav Ustředního Ústavu Geologického* 17. 510 pp. (German summary, pp. 475-95.)
- . 1960b. K současné problematice stratigrafie mladopleistocenních spraší a časového zařazení mladého paleolitu (Aux problèmes actuels de la stratigraphie des loess pleistocènes-récents et de la classification chronologique du paléolithique supérieur). *Archeologické rozhledy*, t. 12, s. 4:560-79.
- LOŽEK, V., and J. KUKLA. 1959. Das Lössprofil von Leitmeritz an der Elbe, Nordböhmen. *Eiszeitalter und Gegenwart* 10:81-104.
- . 1960. Comment. *Current Anthropology* 1:381.
- MENGHIN, OSWALD. 1931. *Weltgeschichte der Steinzeit*. Wien: Anton Schroll and Co.
- MILANKOVITCH, M. 1930. Mathematische Klimalehre und Astronomische Theorie der Klimaschwankungen. *Handbuch der Klimatologie* I. A. Berlin.
- . 1941. *Kanon der Erdbestrahlung und seine Anwendung auf das Eiszeitalterproblem*. Belgrade.
- MOTTL, M. 1951. Die Repolusthöhle bei Peggau und ihre eiszeitliche Bewohner. *Archaeologia Austriaca* 8.
- MOVIUS, H. L. 1960a. Radiocarbon dates and Upper Palaeolithic archaeology in Central and Western Europe. *Current Anthropology* 1:355-91.
- . 1960b. Bas-relief Carving of a Female Figure recently discovered in the Final Périgordian Horizon at the Abri Pataud, Les Eyzies (Dordogne). *Festschrift für Lothar Zotz*, Bonn: Ludwig Rohrscheid, pp. 377-387 and pls. XII-XVI.
- MOVIUS, H. L., JR., and H. V. VALLOIS. 1960. Crâne Proto-Magdalénien et Vénus du Périgordien Final trouvés dans l'Abri Pataud, Les Eyzies (Dordogne). *L'Anthropologie* 63, No. 3-4:213-32.
- MÜLLER, HELMUT. 1953. Zur spät- und nacheiszeitlichen Vegetationsgeschichte des mitteldeutschen Trockengebiets. *Nova Acta Leopoldina* n.f. 110, 16.
- MÜLLER-BECK, HANSJÜRGEN. 1954-55. Zur Datierung paläolithischer Kulturspuren im alpinen Bereich der Schweiz. *Jahrbuch d. Schweizerischen Gesellschaft für Urgeschichte* 44:161-80.
- . 1957. *Das obere Altpaläolithikum in Süddeutschland*. Bonn: Rudolf Habelt Verlag.
- . 1959. Bemerkungen zur Stratigraphie des mitteleuropäischen Jungpleistozäns. *Eiszeitalter und Gegenwart* 10:144-60.

- , 1961. C14-Daten und Absolute Chronologie im Neolithikum. *Germania* 39 (in press).
- MUSIL, R. 1960. *Paläontologische Funde in Sedimenten der letzten Zwischenzeit*. Casopis Moravského muzea, sc. nat., 45.
- MUSIL, RUDOLF, and KAREL VALOCH. 1955. Über die Erforschung der Lössen in der Umgebung von Brünn (Brno) in Mähren. *Eiszeitalter und Gegenwart* 6:148-51.
- , 1956. Spráše Vyškovského úvalu. (Die Lössen der Wischauer Senke.) *Práce Brněnské Závklad. Československá Akademie Věd* 28, No. 6:263-307. (German summary, pp. 302-307.)
- , 1961. Die niederen Terrassen der Svítava bei Brno/Brum. *Práce Brněnské Závklad. Československá Akademie Věd* 28, No. 6:263-307. (German summary, pp. 302-307.)
- MUSIL, RUDOLF, KAREL VALOCH, and V. NEČESANÝ. 1954. Pleistocenní Sedimenty okolí Brna. (The Pleistocene sediments in the vicinity of Brno.) *Anthropozoikum* 4:107-43. (English summary, pp. 157-68.)
- NARR, KARL J. 1951. Terrassen, Lössen und paläolithische Kulturen. *Germania* 29:145-50.
- , 1952. Wenig bekannte Typen und Stationen aus dem rheinischen Magdalenien. *Germania* 30:1-6.
- , 1953. Riss oder Würm? *Germania* 31:125-34.
- , 1954. Formengruppen und Kulturkreise im europäischen Paläolithikum (Stand und Aufgaben der Altsteinzeitforschung). *Bericht d. Römisch-Germanischen Kommission* 34:1-40.
- , 1955. Das Rheinische Jungpaläolithikum: Zugleich ein Beitrag zur Chronologie der späten Altsteinzeit Mittel- und Westeuropas. Beiheft 4 der *Bonner Jahrbücher*. Bonn: Rudolf Habelt Verlag.
- , 1959. C¹⁴ Daten und die Gliederung des Jungpleistozäns. *Forschungen und Fortschritte* 33, Hft. 5:147-51.
- , 1960. Nochmals: C¹⁴ Daten und die Gliederung des Jungpleistozäns. *Forschungen und Fortschritte* 34:90-91.
- NICOLĂESCU-PILOPSCU, C. A., and I. SIRATAN. 1960. Saptaturile de la Tincova. *Materiale si cercetari arheologice* 7.
- PENCK, A. 1882. *Die Vergletscherung der deutschen Alpen*. Leipzig.
- , 1922. Ablagerungen und Schichtstörungen der letzten Interglazialzeit in den nördlichen Alpen. *Sitzungsberichte der Preussischen Akademie der Wissenschaften, Physikalisch-mathematische Klasse*, pp. 214-51. Berlin.
- , 1938. Säugetierfauna und Paläolithikum des jüngeren Pleistozäns in Mitteleuropa. *Abhandlungen der Preussischen Akademie der Wissenschaften* 5. Berlin.
- , 1939. Paläolithische und geologische Chronologie. *Zeitschrift der deutschen geologischen Gesellschaft* 91:57-64.
- PENCK, A., and E. BRÜCKNER. 1909. *Die Alpen im Eiszeitalter*. Leipzig: C. H. Tauchnitz.
- POSER, HANS. 1947. Boden- und Klimaverhältnisse in Mitteleuropa während der Würmeiszeit. *Erdkunde* 2:53-68.
- , 1951. Die nördliche Lössgrenze in Mitteleuropa und das spätglaziale Klima. *Eiszeitalter und Gegenwart* 1:27-55.
- PRADEL, L. 1952. La Grotte périgordienne et aurignacienne du Fontenieux. Commune de Saint-Pierre-de-Maillé (Vienne). *Bulletin de la Société Préhistorique Française* 49, No. 8:413-32.
- , 1953. Considerations sur le Périgordien de la Grotte du Fontenieux. *Bulletin de la Société Préhistorique Française* 50, Nos. 7-8:422-26.
- , 1959. Le Périgordien II de la Grotte des Cottés. Commune de Saint-Pierre-de-Maillé (Vienne). *Bulletin de la Société Préhistorique Française* 56, Nos. 7-8:421-27.
- PROŠEK, F. 1955. Szeletien na Slovensku. *Slovenska Archeologia* 1:148-49.
- PROŠEK, F., and V. LOŽEK. 1957. Stratigraphische Übersicht des Tschechoslowakischen Quartärs. *Eiszeitalter und Gegenwart* 8:37-90.
- REICH, HELGA. 1953. Die Vegetationsentwicklung der Interglaziale von Grossweil und Pfefferbichl im Bayerischen Alpenvorland. *Flora* 140:388-443.
- RIEK, GUSTAV. 1934. *Die Eiszeitjägerstation am Vogelherd im Lonetal*. Bd. I, Die Kulturen. Tübingen, 338 pp., 33 pls.
- , 1960. Das Paläolithikum der Höhlen des Lone- und des Brenztales. *Jahreshefte für Karst- und Höhlenkunde*, Heft I. Stuttgart, pp. 57-104.
- ROGATCHEV, A. N. 1957. Mnogosloinije stoianki kostienkovsko-borchevskogo raiona i problema razvitiia kultury v epokhu verkniego paleolita na Russkoi Ravninie. *Materiali i issledovaniia po arkeologii SSSR* 59.
- RUTKOWSKI, EDMUND. 1959. Plejstocenijskie osady organiczne w Marantowie kolo Konina (Pleistocene organic sediments in Marantów near Konin, Central Poland). *Przegląd Geologiczny* 7, Nr. 6:279-81.
- SAMSON, P., and C. RADULESCU. 1959. Beiträge zur Kenntnis der Chronologie des "Jüngeren Lösses" in der Dobrukscha (Rumänische Volksrepublik). *Eiszeitalter und Gegenwart*, 10:199-204.
- SCHMID, ELISABETH. 1958. Höhlenforschung und Sedimentanalyse. *Schriften des Instituts für Ur- und Frühgeschichte der Schweiz* 13.
- SCHWABEDISEN, H. 1956. Fällt das Aurignacien ins Interstadial oder ins Interglazial? *Germania* 34, Hft. 1-2:12-41.
- , 1957. Das Alter der Federmesserzivilisation auf Grund neuer naturwissenschaftlicher Untersuchungen. *Eiszeitalter und Gegenwart* 8:200-209.
- SHOTTON, F. W. 1959. Insects in Pleistocene deposits. *Journal of the Institute of Biology* 6:3.
- , 1960. Large scale patterned ground in the valley of the Worcestershire Avon. *Geological Magazine* 97, No. 5:404-08.
- SHOTTON, F. W., and I. SIRACHAN. 1959. The investigation of a peat moor at Rodbaston, Penkridge, Staffordshire. *Quarterly Journal of the Geological Society of London* 115:1-16.
- SIMPSON, I. M., and R. G. WEST. 1958. On the stratigraphy and palaeobotany of a late-Pleistocene organic deposit at Chelford, Cheshire. *The New Phytologist* 57.
- SOERDEL, W. 1919. *Löss, Eiszeiten und paläolithische Kulturen. Eine Gliederung und Altersbestimmung der Lössen*. Jena.
- , 1924. *Die diluvialen Terrassen der Ilm und ihre Bedeutung für die Gliederung des Eiszeitalters*. Jena.
- , 1925. Die Gliederung und absolute Zeitrechnung des Eiszeitalters. *Fortschritte der Geologie und Paläontologie* 13:125-251.
- , 1939. Das diluviale System. I. Die geologischen Grundlagen der Vollgliederung des Eiszeitalters. *Fortschritte der Geologie und Paläontologie* 39.
- , 1940. *Die Massenverkommen des Höhlenbären. Ihre biologische und stratigraphische Deutung*. Jena: G. Fischer.
- STEHLIN, H. G. 1922. Revision der Säugerfunde aus Hochterrasse und aus Ablagerungen der grössten Vergletscherung. *Eclogae geol. Helv.* 17:364-77.
- SUETH, H. E. 1960. Secular changes in the concentration of atmospheric radiocarbon. (manuscript).
- TAVERNIER, R., and J. DE HEINZELIN. 1957. Chronologie du Pleistocène supérieur, plus particulièrement en Belgique. *Geologie en Mijnbouw*, n.s., 19:306-09.
- TODÉ, A. 1953. Einige archäologische Erkenntnisse aus der paläolithischen Freilandstation von Salzgitter-Lebenstedt. *Eiszeitalter und Gegenwart* 3:192-215.
- TODÉ, A., F. PREUL, K. RICHTER, et al. 1953. Die Untersuchung der paläolithischen Freilandstation von Salzgitter-Lebenstedt. *Eiszeitalter und Gegenwart* 3:144-220.
- TROLL, C. 1924. Der jungdiluviale Inn-Chiemseegeletscher. *Forschungen z. deutsch. Landeskunde* 23/1.
- , 1959. Kommission für Erdwissenschaftliche Forschung, *Bericht-Jahrbuch 1959 der Wissenschaften und der Literatur Mainz*.
- VALOCH, K., and F. H. BORDES. 1957. Loess de Tchecoslovaquie et loess de France du nord. *L'Anthropologie* 61, No. 3-4:279-88.
- VERILOVA, E. A. 1957. Stoianka Siuren I. *Materiali i issledovaniia po arkeologii SSSR* 59.
- VÉRTES, LÁSZLÓ. 1955a. Paläolithische Kulturen des Würm I/II-Interstadials in Ungarn. *Acta Archaeologica, Academiae Scientiarum Hungaricae* 5, fasc. 3-4:261-77.
- , 1955b. Les conditions de l'interstadial Würmien I/II hongrois éucidées par l'examen des remplissages de grottes. *Acta Geologica* 3, fasc. 4:393-406. Budapest.
- , 1957. *Untersuchungen an Höhlensedimenten. Methode und Ergebnisse*. Magyar Nemzeti Múzeum-Történeti Múzeum, Régészeti Füzetek, ser. 2, 7, 176 pp.
- , 1959. Das Moustérien in Ungarn. *Eiszeitalter und Gegenwart* 3:192-215.
- WEIDENBACH, F. 1956. Zeitliche Einordnung der jungpleistozänen Ablagerungen Mitteleuropas. *Actes du IV Congrès International du Quaternaire* (Rome-Pisa 1953): 1-7.
- WELLEN, M. 1958. Spät- und Postglaziale Florententwicklung im Berner Oberland und Walliser Haupttal. *Veröffentlichungen Geobot. Inst. Rübel zu Zürich* 34.
- WERNERT, P. 1957. Contribution à la stratigraphie paléontologique et préhistorique des sédiments quaternaires d'Alsaces Achenheim. *Mém. Serv. Carte géol. Alsace-Lorraine*, 14.
- WILLIS, E. H., H. TAUBER, and K. O. MÜNICH. 1960. Variations in the atmospheric radiocarbon concentration over the past 1300 years. *American Journal of Science, Radiocarbon Supplement* 2:1-4.
- WOLDSTEDT, P. 1958a. Ein neue Kurve der Würm Eiszeit. *Eiszeitalter und Gegenwart* 9:151-54.
- , 1958b. *Das Eiszeitalter*. 2. Auflage, 2. Band. Stuttgart.
- WRIGHT, H. 1960. Comment. *Current Anthropology* 1:382-83.
- ZOTZ, LOTHAR F. 1951. *Altsteinzeitkunde Mitteleuropas*. Stuttgart: F. Enke. vi + 290 pp., 29 figs.

PHYSICAL ANTHROPOLOGY IN CZECHOSLOVAKIA: RECENT DEVELOPMENTS

by Milan Dokládál and Josef Brožek*

INTRODUCTION

In Czechoslovakia, as in most Central and East European countries, the term "anthropology" refers to the biological study of man, i.e., to *physical* anthropology. However, physical anthropology has traditionally denoted a field substantially narrower than is designated, at times, as "human biology." Thus it does not include such biosocial disciplines as demography or anthropogeography. As taught and studied in Czechoslovakia, anthropology does not include the various branches of science (such as prehistoric archeology, ethnography, linguistics, or comparative studies of religion) which constitute cultural anthropology. In Czechoslovakia, then, anthropology refers to the Science of Man in the narrower sense. It may be defined as the science of human physique, man's phylogenetic evolution and ontogenetic development, and the va-

rieties ("races") of man. It is only natural that in research special attention is being paid to the physical characteristics of the inhabitants of the Czech and Slovak lands, past as well as present.

Physical anthropology has one root in theoretical, the other root in applied biological sciences. In contrast to several European countries, in which institutes and chairs of physical anthropology are incorporated into the colleges of medicine, the Institutes of Anthropology in Czech and Slovak universities are attached to colleges of natural or biological sciences. Nevertheless, in Czechoslovakia heavy emphasis is placed on medical training as a desideratum for physical anthropologists. Important work in physical anthropology is being carried on in medical schools of the universities and in research establishments financed by the Ministry of Health.

It was noted earlier that anthropology in Czechoslovakia does not encom-

pass cultural anthropology. Nevertheless, a number of problems investigated by Czech and Slovak anthropologists are closely related to such fields as archeology and ethnography, and a close collaboration is maintained with these scientific disciplines.

THE GENERAL SETTING

At the time when Czech universities were forcibly closed (November 17, 1939), there existed in Czechoslovakia two Institutes (Departments) of Anthropology, associated with colleges of natural sciences. The Institute in Prague was founded, with roots going back to 1908, by Jindřich Matiegka. In the framework of Charles University it was further developed in collaboration with Jiří Malý, who took over the direction of the Institute in 1932. The Institute of Anthropology in Brno was founded (in 1923) and developed by Vojtěch Suk as a part of Masaryk (now Purkyně) University. Between the two World Wars, most of the research in physical anthropology was concentrated in these two institutes.

In addition, starting in 1928, Karel Absolon began to develop in Brno the museum "Anthropos," designed as an international institute for research on the origin and evolution of man and culture. In Prague the "Museum of Man" was established after the First World War with the aid and financial support of Aleš Hrdlička, the outstanding American physical anthropologist, who was of Czech origin. The Museum was attached to the Anthropological Institute (Department) of Charles University.

* Dedicated to the living memory of Aleš Hrdlička.

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In the Czech lands (Bohemia, Moravia, part of Silesia) anthropological research was also carried on in biological, anatomical, physiological, and hygienic institutes of the medical colleges; in the Pedological Institute of the City of Prague; as well as in connection with investigative work in psychiatry, serology, and the science and medicine of exercise and sports. One of the present authors (J. B.) was first introduced to anthropological research in the Institute of Social Medicine, forming part of the medical services of the former Baťa Shoe Company in Zlín (the present Gottwaldov, in Moravia; see Brožek 1958).

In Slovakia, physical anthropology was not highly developed before 1939. At the Comenius University in Bratislava there existed no independent anthropological research institute, even though in some of the University's departments there were research workers interested in physical anthropology, e.g. in the department of anatomy (directed by Zdeněk Frankenberger) and the department of biology (directed by Josef Babor).

During the war the Czech anthropological institutes, including their directors and staff, fared poorly. The forceful closing of the Czech universities during the German occupation resulted in the cessation of research at the anthropological institutes. The Prague institute was incorporated into the German University of Prague, and the former director, Professor Jiří Malý, was unable to continue his scientific work. At the end of the war the Institute was in a deplorable state. The collections, which had been concentrated mostly in the Hrdlička Museum of Man, as well as the departmental furniture, apparatus, and the extensive library, suffered severe damages and losses.

The fate of the Anthropological Institute in Brno was even more bitter. The building which housed the institute was heavily damaged by a bomb. Its director, Professor Suk, known for his anti-racist views, had to leave Brno to save his life; until the end of the war, he worked as a physician in the local hospital in a small Czech provincial town.

The younger anthropologists, scientific workers and assistants in the two Institutes, also had to interrupt their scientific careers. The majority of them found employment as high school (gymnasium) teachers. Those who had medical training were employed in hospitals and other medical services. Some continued to carry on private anthropological studies. However, there was no possibility of publishing the results of their work. Contacts with investigators

abroad were made more difficult and eventually ceased altogether.

It was a major blow to physical anthropology in Czechoslovakia when the publication of *Anthropologie* was stopped in 1941. The journal was founded in 1923, with the help of Aleš Hrdlička, the founder (1918) and first editor of the *American Journal of Physical Anthropology*. The 19 volumes of *Anthropologie*, published between 1923 and 1941, represent a significant contribution to the science of physical anthropology.

The most painful chapter in the history of Czech anthropology is the breaking up of Absolon's institute and museum "Anthropos" and the destruction of the valuable anthropological collections of the Diluvial division of the Moravian Museum in Brno. The founder of the institute, Professor Karel Absolon, was expelled from the institute and the museum, and both institutions were taken over by the German occupiers. A substantial part of the collections, including many world-renowned paleo-anthropological relics discovered in Moravia's diluvial deposits, was transferred to a castle in the town of Mikulov. Before the German troops left the area, toward the end of the war, the castle was set on fire and a large part of the priceless collections was destroyed. The material destroyed included many of the finds originating in the Moravian mammoth-hunting grounds at Dolní Věstonice, osteological material relating to *Homo předmostensis*, several crania of the neodiluvial man of Mladeč, human skeletal remains from Dolní Věstonice, cranium Brno II, and other rare, irreplaceable items. This material, formerly concentrated in the Brno institutes, had been the pride of Czech anthropology, and had attracted the attention of specialists from all parts of the world. By a happy accident, some objects documenting the primitive art of diluvial man in Moravia were saved from destruction. Thus preserved were the world-famous Venus of Věstonice and a portrait of a woman cut in ivory, invaluable for the study of man's paleohistory and his earliest artistic creativity.

The war-time losses in personnel were also heavy. In Bohemia, two of the initiators of modern Czech anthropology died. These were Professor Jindřich Matiegka (August 4, 1941), the founder of the Anthropological Institute at Charles University, Prague (see Škerlj and Brožek 1952); and Professor Lubor Niederle (June 14, 1944) who, in 1891, was named the first instructor (docent) of anthropology in the Czech lands, and later became professor of prehistoric archeology and ethnology in Prague. In the U.S.A., Aleš Hrdlička also passed away (September 5, 1943). Unquestion-

ably the greatest anthropologist of Czech origin, Hrdlička had spent most of his life abroad, but he was sincerely interested in scientific developments in Czechoslovakia and aided Czech anthropology in many ways.

In the early post-war years all scientific workers, including the anthropologists, were faced with three tasks: (1) reorganizing the research institutes of the universities; (2) training, with minimal delay, the young men and women who were denied university education during the 6 long years of the Occupation; and (3) catching up with developments in science which took place between 1939 and 1945. Relentless effort and long hours of work enabled the Czech anthropologists to make satisfactory progress along all three fronts. The teaching of anthropology was renewed one month after the cessation of fighting, and plans were laid for research with a minimum loss of time.

During the 15 post-war years (1945-1960) the anthropological institutes doubled in number. Equally important was the significant increase in the number of biological and medical research institutes with anthropologists on their staffs. A new generation of Czech and Slovak anthropologists is growing up and is slowly but surely filling the gaps in personnel caused by the war. Unfortunately, the losses suffered by the museum collections cannot be repaired.

At present there are in Czechoslovakia some 30 physical anthropologists with university training actively working in their field of specialization. Research in physical anthropology is carried on in some two dozen different establishments. The productivity of Czech and Slovak anthropologists is well documented by the bibliography compiled by one of the authors of the present communication (Dokládál 1955c) and published by the Anthropological Society in Brno. The bibliography cites 756 publications issued during the first 10 post-war years (1945-1954).

THE INSTITUTIONAL SETTING

Institutions in which anthropologists are active (Figure 1) can be classified into 4 groups: universities (colleges of natural sciences, biology, medicine, and education); special research institutes associated with the Czechoslovak Academy of Sciences and the Slovak Academy of Sciences; Ministry of Education and Ministry of Health; and museums.

COLLEGES OF NATURAL SCIENCES AND BIOLOGY

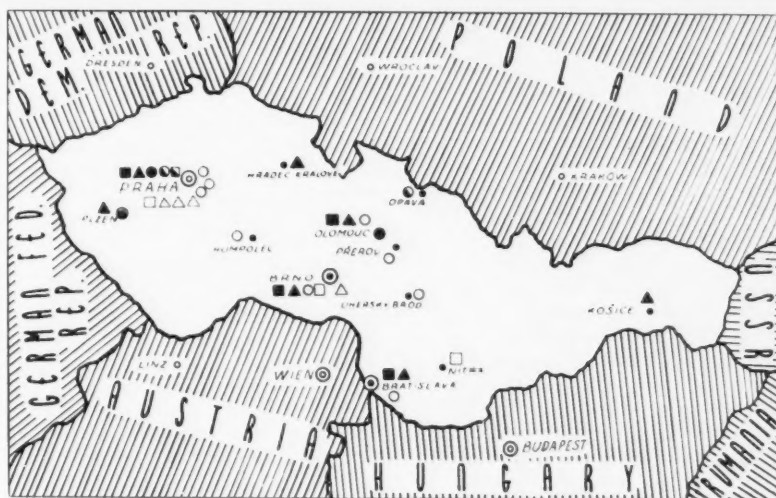
In 4 universities (Charles University in Prague, Masaryk [now Purkyně] University in Brno, Palacký University in

Olomouc, and Comenius University in Bratislava) there are anthropological institutes, associated with the colleges of natural or biological sciences. The addition of a college of natural sciences to the Šafařík University in Košice, which is expected to have its own institute of anthropology, is planned.

Anthropological Institute, College of Biological Sciences, Charles University, Prague. This institute was the first of its kind to be established in the Czech lands. It began, modestly, as a "cabinet" in 1908. In 1918 it was transformed into an independent institute, and was directed by Professor Matiegka until 1932, when Professor Malý became its head. Today the Institute is well equipped with instruments, including those needed for research in "physiological anthropology," and has adequate library facilities. While it suffers from a shortage of space, it is one of Europe's largest and best-equipped anthropological institutes. It provides facilities for research in all fields of physical anthropology.

After the war the Institute was again directed from 1945 to July 1950 by Professor Malý. Since his death, docent Vojtěch Fetter has been in charge; he is assisted by Chruďoš Troniček, Svatava Titlbachová, and Karel Hajniš. Former students, including pre-war and post-war assistants of the Institute, are pursuing anthropological research in numerous institutions.

While investigations were carried out in various areas of anthropology during the 15 post-war years, studies in physical (somatic), forensic, and ethnic anthropology predominate. Anthropological research is being done on the skeletal remains of outstanding individuals. Material from crypts, churches, and ossuaries is being worked on. In connection with criminal investigations, individuals are sometimes identified with the aid of anthropological methods. Close collaboration is maintained with various medical establishments in connection with the assessment of physical development of children and adolescents. Plastic reconstructions of the head and the face of man's evolutionary ancestors and of the historical inhabitants of the Czech lands have been prepared in collaboration with sculptors. The Institute has organized several field expeditions (including the important field studies in Silesia of 1947 and 1949), as well as detailed anthropometric investigations of forest workers from all areas of Czechoslovakia, and the anthropometric study of several thousand participants in the 1955 all-state athletic games ("Spartakiáda"). The Institute collaborates with light industry in standardizing the dimensions of various products. In the past years increased attention has



Meaning of Symbols:

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|---|-------------------------------|
| ■ University Anthropological Institutes (Departments) | ● Pedagogical Institutes |
| ▲ Departments of Human Anatomy in Medical Schools | ◌ Institute of Sport Medicine |
| ● University Biological Institutes | ◌ Museums |
| | ◌ Archeological Institutes |
| | ◌ Various Research Institutes |

FIG. 1. Location of institutions in Czechoslovakia in which anthropologists are active.

been paid to the anthropometry of athletes and to certain problems of racial typology. Lecture outlines of anthropology and a laboratory guide were prepared. The workers of the Institute also take part in the popularization of their discipline. Several exhibits were arranged, concerning the origin and evolution of man, human races, and the work of Aleš Hrdlička. Several popular publications were devoted to human evolution and to the varieties of mankind.

Anthropological Institute, College of Natural Sciences, Masaryk (now Purkyně) University, Brno. During the war the Institute suffered heavy losses, especially as regards its serological and anthropometric laboratories, and its collections which had been enriched by many objects gathered in various parts of the world by the founder of the Institute, Professor Suk. Since the old building was badly damaged, the Institute was moved after the war into modern and ample quarters. Fortunately, Professor Suk, the dean of Czech anthropologists, was able to return to work, and he took energetic steps toward the renewal of teaching and research. He was ably aided by his student and co-worker, docent (today professor) Jindřich Valšík. In 1949, when Professor Suk reached 70 years of age and retired, Dr. Valšík took over the direction of the Institute and the lectures in anthropology. With the help of the younger men,

the Institute was soon rebuilt. Today it is well equipped, and its library collections are fairly representative.

The younger generation of anthropologists is active along several directions, with special emphasis on three topics: 1) Anthropological research on the population of the Czech lands, with special attention to Moravia and Silesia, is being continued; in this field the Brno Anthropological Institute has had a good record of performance in the pre-war years. 2) A second group of investigators is working in dactyloscopy, with special reference to its applications in the determination of paternity. 3) A number of research workers are concerned with the problems of physical development of children, including the changes brought about by the war and those taking place during the post-war years. In addition to these main topics, studies have been made of sex differences in physique and of age changes.

In 1954 Professor Valšík was called to the Comenius University in Bratislava with the assignment of developing a new Institute of Anthropology. The Brno Institute has remained in the charge of Anna Lorencová, who in recent years has been especially interested in prehistorical and historical anthropology. She is working on the skeletal materials obtained in connection with archeological investigations undertaken in Moravia, especially the material dat-

ing from the times when the Czech lands were first settled by Slavic peoples. She collaborates closely with investigations carried out in southern Moravia by the Prehistorical Institute of the College of Philosophy in Brno. Her specialty is the study of deformed crania found in the Czech lands.

Professor Suk, who reached the age of 80 on September 18, 1959, lives in retirement in Brno. For his lifelong work in anthropology he was decorated with the Order of Work. He continues his scientific labors, and recently published a major work on the equality of human races. He follows with interest the developments in anthropology in Czechoslovakia and abroad, and takes part in anthropological conferences and in the meetings of the Anthropological Society, which he helped to found.

Anthropological Institute, College of Natural Sciences, Palacký University, Olomouc. This was the first anthropological institute established following the Second World War, when in 1946 the University was renewed in the ancient Moravian city of Olomouc. The Institute was called at first the Institute of Child Biology, and for some time it was associated with the College of Education. Since 1959 the Institute has been part of the Palacký University. The Institute was developed by Dr. V. Fetter, on "loan" from Prague who, in the early post-war years, gave lectures in Olomouc on anthropology and child biology. In 1947 Dr. Jaroslav Šmířák joined the Institute, which is now modestly equipped, with focus on research on living man. In addition to teaching, members of the Institute carry out research, mostly in collaboration with the Anthropological Institute of Charles University in Prague, on several topics: Silesian school children, industrial workers of Silesia, and forest workers. In recent years the emphasis has shifted to the physical growth and development of children and adolescents.

Anthropological Institute, College of Natural Sciences, Comenius University, Bratislava. Lectures in anthropology were initiated in Bratislava after 1945. Professor Jiří Malý, from Prague, served as the first lecturer. After his death E. Stekláčová, from the Anatomical Institute of the College of Medicine in Bratislava, helped out for a time. In 1952 the lectures were taken over by Professor J. Vašík, who in 1954 moved permanently from Brno to Bratislava, with the assignment of establishing, first a cabinet, and later (1955) an Institute of Anthropology. The Institute has the necessary basic equipment and is being steadily enriched. Its head is assisted by Milan Černý, Milan Pospíšil, Mária Drobná-Krůtčlová, and Matej Hanulík. Thus, as regards personnel, the

Institute is the largest anthropological establishment in Czechoslovakia. Research is carried on in four directions: somatology of Slovak school children; ethno-anthropological investigations in Slovakia; osteology; and dactyloscopy and dermatoglyphics. The personnel collaborates closely with the Anatomical Institute of the College of Medicine in Brno.

COLLEGES OF MEDICINE

In Czechoslovak colleges of medicine, which administratively are a part of the universities, there exist no independent institutes of anthropology. Nevertheless, active work in this field is going on, especially in several institutes of anatomy and biology, but also in some of the clinics.

Anatomical Institute, College of Medicine, Charles University, Prague. Professor Ladislav Borovanský, director of the Institute and dean of Czechoslovak anatomists, is well known among anthropologists for his pre-war studies, carried out in collaboration with his associates, on the growth and ossification of bones in human males from birth to adulthood. He has published a monograph on sex differences in human crania. Two of his present assistants are active in the borderland between anthropology and anatomy: Milan Doskočil (physical development of infants, osteology) and Radomír Čihák (anthropology of soft tissues—comparative myology).

Anatomical Institute, College of Medicine, Masaryk (now Purkyně) University, Brno. The Anatomical Institute in Brno provides very favorable conditions for research in anthropology. The director of the Institute, Professor Žlábek, has always been interested in physical anthropology, as can be seen from the fact that he obtained a doctor's degree in natural sciences in addition to his M.D. Today he is principally interested in microscopic and functional anatomy. However, his many earlier studies in comparative myology have significance not only for the anthropology of the soft tissues in man but for the study of primates in general. His continued interest in physical anthropology is attested by his serving as president of the Brno Anthropological Society. Osteological studies carried out in the Institute have concerned such topics as the effect of the loss of teeth on the shape and size of the human mandible (V. Janáček, I. Malec 1953), and the development of the sulcus nervi ulnaris in the humerus (J. Filkuka, Z. Vlašín 1953). In recent years variations of the human skull have been investigated systematically (E. Svobodová 1953, V. Pokorný 1953, K. Balabánová, Z. Šťastný, J. Vašíček, V. Zavřelová 1955).

Since 1953 Milan Dokládál, assistant in the Institute, has been principally concerned with physical anthropology. Besides *ad hoc* topics, his research has dealt primarily with two problems: (1) the relative stability of human racial characteristics and the influence of various conditions upon them; (2) the anatomy of human growth. During his association with the Anthropological Institute in Brno he was also concerned with the ethnic anthropology of Moravia.

Anatomical Institute, College of Medicine, Charles University, Hradec Králové. Professor Jan Hromada, the director of the Institute, had previously analyzed skeletal materials from Bohemian ossuaries. The assistant in the Institute, Přemysl Poláček, studied the interrelations between physical characteristics of man at the Anthropological Institute in Brno. At the present time, material is being gathered in the Institute for osteological studies on primates. The junior assistant of the Institute, Helena Malá, is specifically concerned with physical anthropology and is working on skeletons from a variety of prehistoric burial grounds.

Anatomical Institute, College of Medicine, Palacký University, Olomouc; Anatomical Institute, College of Medicine, Charles University, Plzeň. At the present time, no technically trained anthropologists are on the staff of either of these institutes. However, some of the studies carried out in recent years are relevant for anthropology. In Plzeň, Jiří Heřt and Václav Novák investigated the growth of the long bones, the localization of nutrient foramina in bones, and the morphological differences between the individual metacarpal and metatarsal bones. In regard to the work of the anatomists in Olomouc, reference should at least be made to the investigations on the development of septum humerale and foramen supra-trochleare of the humerus, carried out by Josef Zrzavý, director of the Institute, and his co-workers. In recent years some members of the Institute's staff (Hrnčíř, Lisoněk) have been engaged in research on child development.

Biological Institute, College of Medicine, Charles University, Prague. In addition to his concern with other problems, the director of the Institute, Bohumil Sekla, is interested in human genetics. As long ago as 1937, Sekla and one of us (J. B.) discussed plans for joint somato-psychological investigations on twins. Unfortunately, the march of politico-historical events, with the partial (1938) and eventually total (1939) occupation of Czechoslovakia, made it impossible to carry out these plans. In 1946 Professor Sekla published a volume on "Heredity in Nature and in Society." He serves as legal expert for

biological tests of paternity, and is studying their theoretical basis and their reliability. In this field he has the collaboration of Lidmila Malá, widow of Professor Jiří Malý. Dr. Malá is also interested in the heredity of fingerprints.

Biological Institute, College of Medicine, Charles University, Plzeň. Both Professor Bohumil Krajník, the director of the Institute, and Evžen Strouhal, one of the assistants, have been active in anthropological research. Professor Krajník had already become well known in the pre-war years for his propagation of constitutional studies. He was one of the founders (1936) of the Czechoslovak Biotypological Society. When one of us (J. B.) was leaving for the U.S. in 1939, he brought with him, on Dr. Krajník's request, a volume of studies published by the Biotypological Society and dedicated to Aleš Hrdlička. There was a strong branch of the Biotypological Society in Zlín (the present Gottwaldov) which owed much to Dr. Krajník's enthusiasm. We (J. B.) remember with pleasure the stimulating meetings of the Zlín branch of the Society, in one of which we had the privilege of discussing statistical methods applied in research on human biology.

Among the larger studies undertaken by Professor Krajník in recent years, we may mention his extensive anthropometric investigation of boys and girls in the 11 to 18 year age-range and the study of their physique with reference to constitutional types. This work has not yet been published. Dr. Strouhal collaborates with Professor Krajník in the evaluation of methods and indices used for the characterizations of constitutional types. His personal interest is the field of prehistoric anthropology. At present he is engaged in the study of morphological characteristics of dentition and dental pathology, especially the formation of dental cavities, in the prehistoric inhabitants of the Czech lands.

The Institute for Medicine in Physical Education, College of Medicine, Charles University, Prague. The division of anthropometry, established in this Institute in 1949, specializes in the anthropometry of sports and exercise. The division was developed by Zdeněk Novák, who was succeeded by Vladimír Novotný in 1954. The principal task of the division is the study of the morphological changes typical for certain sports and resulting either from faulty training techniques or one-sided exercise. On the basis of the study of these shortcomings, means are sought for their prevention. Attempts are also made to define the complexes of somatic characteristics typical for individual sports, and to clarify the concept of "sport

types." Today the interest is shifting more and more from a static to a dynamic anthropometry (see Count 1955), and emphasis is placed on somatic changes resulting from physical activity. Here anthropometric research has close contacts with the investigations of sport physiologists.

Anthropology in clinical establishments associated with the colleges of medicine. Some of these establishments, such as the Children's Clinics, plan to employ professionally trained anthropologists. While this is a project for the future, a number of physicians have technical knowledge of physical anthropology and apply it in their work. Thus Ferdinand Skaloud (1949), professor of orthodontia in Prague, emphasizes anthropological data and points of view in dentistry.

Anthropometric examinations are essential in the characterization of children's health status. Several publications have been concerned with this topic. In recent years, Marie Nováková, a research anthropologist in the Division of Rehabilitation of the First Children's Clinic, College of Medicine, Charles University in Prague, has been studying the formation of the chest in children, and its deformations resulting from different pathological processes. In the out-patient department of the Children's Division of the Polyclinic in Prague, Milada Horáčková and Zdenka Kumcová, both physicians, are studying disorders of growth from the point of endocrinology. In the Balneological Research Institute at Mariánské Lázně, V. Křížek examined the relations between different types of nephrolithiasis and somatic characteristics, especially the eye color. In Brno, Karel Kubiček, a school physician, and his wife, Božena Kubičková, a physician in the Children's Hospital in Brno, have long been interested in the systematic anthropometric follow-up of school children and have devoted a number of publications to this subject. Pavol Andrik is introducing anthropological methods in dentistry at the Stomatological Clinic, College of Medicine, in Bratislava.

Several doctors of medicine are engaged in the study of paleopathology of bones discovered in the course of archeological field investigations. Among these are Otakar Hněvkovský, professor of orthopedics in Prague, and Mojmir Fait, assistant in the Orthopedic Clinic of the College of Medicine in Brno. Two assistants in the dental clinic of the College of Medicine in Brno, Boris Bílý and Jarmila Pavliková, are studying the incidence of dental cavities among the prehistoric inhabitants of the Czech lands.

Research in anthropology is also being carried on in several institutes of occupational medicine (e.g. Eduard Bena in Prague), and in hematological departments and blood transfusion centers (studies in sero-anthropology).

Institute for the Organization of Health Care (formerly the Institute of Social Medicine), College of Medicine, Charles University, Prague. The director of the Institute, Professor Václav Prošek, is principally concerned with problems of social hygiene and social medicine, but he has had a long-standing interest in anthropological problems, especially the physical development of children. It may be appropriate to note his detailed study of the influence of the depression of the 1930's on the physical development of children (V. Prošek 1936). After 1945 Professor Prošek and his co-workers organized several smaller studies on the physical development of school children. He participated as a consultant in other undertakings of this kind, especially the all-state anthropometric investigations of youth in 1951. Marie Hrubcová, assistant of the Institute, is engaged in the systematic study of social anthropology, including the influence of the family environment and nutrition on child development.

COLLEGES OF EDUCATION

(after September 1, 1959, Pedagogical Institutes.)

Lectures in somatology, including the rudiments of physical anthropology, are given in all of the 19 Pedagogical Institutes in which future teachers for elementary and intermediate schools are being trained. However, specialists in anthropology are active in only two of these institutes, located in Prague and Opava.

Chair of Biology, Pedagogical Institute, in Prague (formerly the High School of Education). The chair of biology of the Pedagogical Institute has had divisions of biology, botany, and zoology. In 1954 a division of anthropology was established, under the direction of Jaroslav Suchý. In addition to his teaching responsibilities, Dr. Suchý is devoting attention to the study of the somatic development of school children; regional ethnic anthropology of Czechoslovakia; and anthropological typology. He collaborates closely with the Anthropological Institute of Charles University, where he received his training and formerly served as assistant.

Chair of Biology, Pedagogical Institute, Opava (formerly Higher School of Education in Opava). Physical anthropology is professionally repre-

sented at the Institute by Ladislav Crhák, trained in Brno under Professor Suk. He is interested principally in the physical anthropology of living man, with special reference to the regional problems of Silesia and to the somatology of school children. He is also associated with the Division of Natural Sciences at the Silesian Institute of the Czechoslovak Academy of Sciences in Opava.

ACADEMIES OF SCIENCES

The Czechoslovak Academy of Sciences is the highest scientific body of the country. In collaboration with the universities and the research institutes associated with the various Ministries, it plans, directs, and co-ordinates all the scientific work in Czechoslovakia. In reference to the specific problems of Slovakia, this function is filled by the Slovak Academy of Sciences. At the present time some 90 research establishments are active in the framework of the Czechoslovak Academy of Sciences, while some 30 institutes in the various fields of science are associated with the Slovak Academy of Sciences. So far no independent institutes of anthropology have been established in the framework of either Academy. However, several of the institutes in the biomedical division, as well as institutes of archeology and ethnography, deal with problems related to anthropology. Specialists in physical anthropology are on the staffs of the institutes of archeology located in Prague, Brno, and Nitra.

The Institute of Archeology, Czechoslovak Academy of Sciences, Prague. Jaroslav Böhm, the director of the Institute, has always shown understanding and sympathy for anthropological research. Being fully aware of the significance of anthropology for archeology, he established in 1950 an independent division of anthropology at the Institute of Archeology. The principal task of the division, headed by Jaromír Chochol, is the preservation and scientific analysis of the material obtained in connection with archeological excavations. Hana Palečková serves as assistant in the division. The work of the division is proceeding in three directions: ascertaining the physical characteristics of populations that inhabited the territory of Czechoslovakia throughout its history; examining functional changes, principally those associated with occupational activities, as well as such abnormalities and pathological processes as may be reflected in skeletal materials; and investigating the phylogenetic development of man.

Evžen Vlček, now full-time in Prague,

was formerly associated with the division of anthropology, Archeological Institute, Slovak Academy of Sciences in Nitra, in Slovakia. Dr. Vlček, a physical anthropologist with medical training, has been concerned with a broad spectrum of problems in physical anthropology, including research on the living man. Nevertheless, in his intellectual *curriculum vitae*, studies in paleo-anthropology predominate, including research on the predecessors (*Homo primigenius*, *Homo sapiens fossilis*) of present-day man, and on Tertiary simians once living in the territory of Czechoslovakia. In the summer of 1958 Dr. Vlček participated as anthropologist and physician in an archeological expedition to Mongolia, supported by the Czechoslovak Academy of Sciences. The goal of the expedition was to study the old Turkish monument of Prince Kültegin on the River Orkhon, dating from the 8th century A.D. Dr. Vlček had the opportunity to study this early skeletal material, as well as to examine several smaller groups of the present population of Mongolia.

Archeological Institute, Czechoslovak Academy of Sciences, in Brno. The Institute carries on archeological investigations in Moravia. Anthropology is represented by Milan Stloukal, trained in Prague. Dr. Stloukal takes care of the skeletal material obtained in excavations undertaken by the institute. Personally, he is especially interested in the Old Slavic skeletons from Mikulčice, dating from the 8th to the 10th century. A part of the skeletal material obtained by the personnel of the Institute, especially the skeletal remains of *Homo sapiens fossilis*, was transferred to the Moravian Museum in Brno for analysis by Jan Jelínek. Jan Pavelčík, associated with the Comenius Museum in Uherský Brod, has worked on parts of the anthropological material going back to the period of Great Moravia (about the 9th century A.D.) and obtained in excavations at Velehrad and Staré Město in southeastern Moravia, near Uherské Hradiště.

Anthropological Institute, Slovak Academy of Sciences, in Nitra. The anthropological division of this Institute was developed by Evžen Vlček, who later moved to Prague. During his association with the Nitra institute, Dr. Vlček paid special attention to the paleo-anthropology of Slovakia and amassed much skeletal material. Unfortunately, the Nitra collections remain partly unanalyzed, since the Institute is at present without a scientist competent to work on these problems and interested in them. Cyril Ambroz, trained in anthropology in Brno, is focusing his attention on the osteology of domestic animals, the skeletal re-

mains of which were and are being found with human skeletal remains in the burial grounds. At present, emphasis is being placed on this topic, since the animal bones found in the ancient burial grounds supplement the information concerning the mode of life of prehistoric man. In this way we learn about the kinds of animals that were hunted by man at that time, and about the distribution of the animals in specified areas and periods of time. The skeletal remains dating from later periods tell us which animals were domesticated and raised by man. Their skeletal development, furthermore, documents the ways in which their bony structure was changing in the process of domestication.

Silesian Institute, Czechoslovak Academy of Sciences, Opava. This is a regional research institute devoted to systematic investigations of social, economic, cultural, and natural conditions in Silesia. The Division of the Natural Sciences does not have on its staff a specialist in anthropology, but such an opening is included in the Institute's personnel plans and should be filled shortly. At present Ladislav Crhák, from the Pedagogical Institute in Opava, is collaborating with the Silesian Institute. Charged with supporting and co-ordinating research concerning nature and the people inhabiting that section of Silesia which forms a part of the Czechoslovak Republic, the Silesian Institute is providing financial support for the work of investigators who are associated with other institutions but carry on their field work in Silesia.

MINISTRY OF HEALTH

Two of the institutes under the jurisdiction of the Ministry of Health employ professionally trained physical anthropologists: The Institute for the Care of Mother and Child, and the Institute of Hygiene.

Institute for the Care of Mother and Child, Prague-Podolí. The Pediatric Division of the Institute is engaged in physiological and pathological investigations on infants and very young children (from birth to the age of 3 years). Somatic and mental development, and the nutrition of children in this age group, represent some of the principal research topics. Availability of representative, reliable somatometric data is crucial for pediatric practice and research. Since growth and development represent fundamental biological phenomena and are of importance as indicators of the child's health status, it is hardly surprising that an anthropological research group was established in

the framework of the Pediatric Division of the Institute. It is headed by Alena Šobová, and its principal task is to study the physical growth and development of the child during its first 3 years of life, with special reference to the somatometric and qualitative morphological characteristics. A substantial amount of work has been accomplished since 1954, the year in which the research group was established. Its most important single achievement was the compilation of information, in tabular form, regarding the growth of children in height and weight. The tables were found useful by pediatricians for the appraisal of the young child's physical development. In addition, the results found uses in industry in connection with the standardization of clothing and the manufacture of furniture designed for children.

The Institute of Hygiene, Department of School Hygiene, Prague. A division for the morphology of the school child was established in the framework of the Institute in 1954. The division is in the charge of Miroslav Prokopec, formerly assistant in the Anthropological Institute of Charles University in Prague. The research program is roughly identical with that of the anthropological research group in the Institute for the Care of Mother and Child, except that it is concerned with children and adolescents in the age range from 3 to 18 years. New guides, in tabular and graphic form, were developed for a rapid appraisal of the somatic development of school children for purposes of medical practice. The graphic method of presentation, developed by M. Prokopec and V. Kaplan, considers height and weight simultaneously, and enables one to evaluate the child's weight in reference to his height, to compare his position in regard to these characteristics in groups of the same age, and to follow effectively the changes in height and weight and their relation. The Division is cooperating with the *Centre International de l'Enfant* in Paris.

MINISTRY OF EDUCATION

J. A. Comenius Pedagogical Research Institute in Prague. The personnel of the biological division and of the division of hygiene of this Institute (A. Vodička, J. Ričář, V. Hladká, J. Meissner, and others) have organized, since 1945, a series of studies on the physical development of school children. The results were published in the voluminous *Collection of Somatometric Studies*, edited by Vodička and published in 1954.

Věra Hladká has been engaged in

extensive research on the anatomical and physiological characteristics of the physical development of children aged 7 to 11 years. Josef Wolf is concerned with problems of evolution, including man, and is active in bringing the accomplishments of anthropology to the attention of the public, especially the school children. He takes part in the development of the course outlines in biology for use in non-technical schools. He defends the point of view that anthropology should be accorded the same place in the teaching of biology that has been accorded to botany and zoology.

Physical Education Research Institute, Prague. In the physiological division of this institute, Jana Pařízková is introducing new approaches to the study of human physique based on the point of view that height and weight are inadequate criteria of somatic growth and development, since they do not enable us to analyze body weight in terms of its components (skeleton, musculature, adipose tissue, plus viscera and skin). In this she takes into account modern methodological developments in the study of body composition, and continues a tradition going back to Jirí Matiegka, who in 1921 described a method for the partition of body weight on the basis of somatic measurements (cf. Škerlj and Brožek 1952, Brožek 1956a). Traditional body measurements are supplemented by data on skin-fold thickness and total body volume, determined by underwater weighing.

MUSEUMS

Up to the present time, independent divisions of anthropology have not been established in association with either the state or provincial museums. However, osteological specimens are regularly displayed in connection with archeological exhibits. The development of man is frequently documented by reconstructions of the crania, the heads, and the bodies of the most significant evolutionary predecessors of contemporary man. Some of the museums are of special importance for anthropology because of the extensive skeletal material deposited in them. This is true in particular of the Moravian Museum in Brno and its treasures concerning Pleistocene man in Moravia, insofar as they were not destroyed during the war. The National Museum in Prague contains paleoanthropological materials found at different sites in Bohemia and also at Gáncé in Slovakia. The Náprstek

Museum of General Ethnography in Prague contains rich anthropological and ethnographic collections. The materials were gathered from all corners of the world by Czech explorers, ethnographers, and anthropologists, and represent valuable documents for ethnographic and ethno-anthropological studies. Reference was made earlier to the Hrdlička Museum of Man, associated with the Anthropological Institute of Charles University in Prague, which is of special importance for physical anthropology.

It may be noted that numerous regional and county museums have significance for anthropology. Thus in the County Museum of Humpolec, M. Prokopec helped to develop a permanent exhibit devoted to the life and work of Aleš Hrdlička, born in Humpolec. The County Museums in Olomouc and in Přerov contain valuable materials from the well-known diluvial site of Předmostí. At present, anthropologists are on the staffs of only two museums, the Moravian Museum in Brno and J. A. Comenius Museum in Uherský Brod.

Moravian Museum in Brno. Jan Jelínek, the director of the Museum and the principal investigator in the Division of Diluvial Studies, specializes in anthropology. Since 1950, when he joined the Museum, he has been able to bring together rich skeletal remains dating from various archeological periods. The material was obtained in part in the course of excavations undertaken by the Museum itself (especially the skeletons from the Bronze Age), and part of it was given to the Museum by the Archeology Institute. Dr. Jelínek is interested in several problems: prehistoric populations of the Czech lands; skeletal characteristics of Pleistocene man, especially in Moravia; morphological variations in human skeletons with reference to racial origin; paleopathology of the prehistoric populations; and paleontology of primates. In collaboration with other specialists in the Division of Diluvial Studies and the Geologico-Paleontological division, he is participating in excavations carried on at various diluvial sites in Moravia. These excavations are undertaken principally for purposes of verification, but sometimes they yield finds significant for research on diluvial man and his culture.

Within the framework of the Moravian Museum, efforts are being made to rebuild the museum and institute "Anthropos," founded by Professor Absolon, and all but destroyed during the war.

J. A. Comenius Museum in Uherský

Brod. The Museum, established to commemorate J. A. Comenius, the founder of modern education, who was born in 1592 (Spinka 1943), is directed by Jan Pavelčík, who served before the Second World War as Professor Suk's assistant in Brno. He is systematically studying the anthropological and ethnographic features of Moravian Slovakia, and analyzing skeletal material dating from the period of Great Moravia (about the 9th century), which was excavated in Staré Město, Modrá near Velehrad, and in a number of burial grounds of lesser significance. He is also engaged in the study of the racial typology of the people of Moravian Slovakia, a region in southeast Moravia.

MAJOR AREAS OF RESEARCH

PALEOANTHROPOLOGY, PREHISTORIC AND HISTORIC ANTHROPOLOGY.

From the point of view of paleoanthropology, the territory of Czechoslovakia is a veritable treasure house (Figure 2). Numerous indications that these regions were inhabited by men of the Neanderthal type and by *Homo sapiens fossilis* came to light in the latter part of the 19th century. Soon it became apparent that during the Upper Paleolithic period the area now constituting Czechoslovakia was one of the important centers of mankind in Europe. In particular, Moravia, the central part of Czechoslovakia, was so situated that it formed a crossroads for the migrations of various groups of Pleistocene men. The mammoth hunters, of Cro-Magnon type, entered the territory through the Moravian Gate and followed the broad river valleys. The skeletal remains of fossil man which have been found in Brno, Předmostí, Mladeč, and Dolní Věstonice are of outstanding significance in European paleoanthropology.

It is ironic that many of the remains of diluvial man found in Moravia were destroyed during the Second World War by members of the species *Homo sapiens*. Only heaps of bone ashes remained. Many documents of material culture, originating from such important localities as Předmostí and Dolní Věstonice, were also destroyed. In addition, the materials from the cave of Šipka and from Mladeč were severely damaged.

Thus at the end of the war the paleoanthropologists of Czechoslovakia were faced with the difficult task of making up these losses. It was fortunate that the Archeological Institute of the Czechoslovak Academy of Sciences was provided with enough financial support to engage in an intensive and systematic search for paleoanthropological



Meaning of Symbols:

△ Places where remains of Neanderthal man (*Homo primigenius*) were found.

■ Places where remains of fossil man (*Homo sapiens fossilis*) were found.

FIG. 2. The most important paleoanthropological localities in Czechoslovakia.

materials, using the most modern field methods. These efforts were crowned with results which are important for physical anthropology as well as for archeology. In the past 15 years, discoveries were made of the skeletal remains of fossil man which bring the number of individual skeletons close to 60 and the number of sites to 14. In this regard Czechoslovakia assumes one of the most significant places in the annals of anthropology.

The majority of the finds come from Moravia. The Moravian Museum in Brno and the Brno branch of the Archeological Institute of the Czechoslovak Academy of Sciences are carrying on systematic investigations of selected localities that were once inhabited by man during the Pleistocene, especially at Dolní Věstonice in southern Moravia, and more recently at Pavlov. Confirmatory investigations were made at certain other sites which had previously been studied (Brno, Mladeč).

B. Klíma (1950), who is continuing the investigations previously directed by Professor Absolon, discovered in 1949 the grave of a woman, covered by the scapulae of a mammoth. An anthropological description of the skeleton, designated as Dolní Věstonice III, was provided by Jelínek (1954). The skeleton was that of a very slender, short, middle-aged woman. The morphological characteristics identify it definitely as an advanced type of *Homo sapiens fossilis*.

Geologically, the skeleton dates from the beginnings of the period Würm III. It resembles most closely the female calvaria designated as Dolní Věstonice II, found in 1930 in the same locality. With the skull in *norma verticalis*, the male calvaria Dolní Věstonice I, found in 1925, is decidedly pentagonoid and thus differs from the ovoid form of the

calvaria Dolní Věstonice III. By means of new methods of dating, using radioactive carbon C-14, the age of the skull and of the site itself has been estimated at about 26,000 years. During the post-war years, several ancient human teeth were found at Dolní Věstonice. These were also described by Jelínek (1952).

Explorations at the new site at Pavlov, about one kilometer from Dolní Věstonice, began in 1952. This locality is rich in artifacts, and there are good reasons to expect further finds of skeletal remains of fossil man. In the fall of 1957 Dr. Klíma discovered the skull of a man about 40 years of age. A detailed description has not yet been published, but even a cursory examination (Jelínek 1958) established that the skull exhibits a number of archaic features. These include powerful supraorbital arches, markedly developed ridges for the attachment of the neck muscles and the muscles of mastication, and an overall impression of massiveness. Nevertheless, the specimen as a whole is morphologically modern. The skull differs from the male calvaria Dolní Věstonice I (1925), which has a finer structure, and is closer to the male skulls Brno II and Předmostí III. By means of C-14 the age of the Pavlov site was estimated at about 25,000 years. Thus it is about contemporaneous with the neighboring locality of Dolní Věstonice, or only slightly younger. It is interesting to note this morphological heterogeneity in populations at about the same chronological horizon.

Another discovery of remains of early man was made in Moravia in 1949 by V. Hrubý, associated with the Moravian Museum, during the excavation at Staré Město near Uherské Hradiště. The skeleton appears to have been that of a middle-aged woman of relatively short

stature and well developed musculature. Jelínek (1956) characterized the skull as a mesencephalic type of *Homo sapiens fossilis*. Geologically it is dated as belonging to the end of the period Würm III. The skeleton presents important similarities, on the one hand, with the mesencephalic skulls of mesolithic man from Ofnet and Tévec and, on the other hand, with the skulls found at Brno and Dolní Věstonice.

In contrast to Moravia, the territory of Bohemia has been incomparably poorer in human remains from the late Pleistocene. After the war some remains were found in the cave Zlatý Kůň near Beroun in the Karst region of Central Bohemia. The skeletons, designated as Zlatý Kůň I, II, and III, were discovered between 1950 and 1953 by the archeologist F. Prošek. A preliminary description was provided by Vlček (1952, 1957), who noted a number of primitive features. A detailed anthropological analysis has not yet been made available. Geologically these skeletal remains date from the period Würm II; culturally they belong to the older phase of the Upper Paleolithic period.

There are several older finds which were re-examined with reference to geological dating and evolutionary status. Vlček (1949, 1950, 1952, 1953, 1955, 1956, 1958) studied in detail the endocranial cast of the skull found at Gánovce in Slovakia in 1926. In 1955 Vlček found evidence of two hominoid long bones (radius and ulna) at the same site. The remains of the man of Gánovce go back in time to the end of the last interglacial Riss-Würm period. Morphologically they belong to an early form of Neanderthal man, sometimes referred to as *Homo praeneanderthalensis*, characterized by a small cranial capacity, and known also from Krapina, Ehringsdorf, and Saccopastore.

On the basis of the data obtained during the re-excavation of the locality which had yielded the skeletal remains designated as Brno II, it was possible to confirm that this find belongs to the period Würm II and represents a primitive type of *Homo sapiens fossilis*, similar to the French Combe Capelle find (Jelínek 1958). The remarkable male statue made of ivory, which was found at the site, indicates that Brno II man was able to express himself artistically with a surprising degree of perfection (Jelínek 1957, 1958). Jelínek's investigations indicate that the Moravian finds of fossil man, dating from the older phase of the Upper Paleolithic period, suggest hybridization of anatomically modern man with a considerably more primitive, pre-Neanderthal type. As far as we are aware, the West

European finds do not show analogical features.

At Svatý Prokop near Prague a skull was discovered in 1887 by J. Kočenský. Its placement on mankind's evolutionary ladder has long been disputed. Vlček (1951) re-examined the skull and came again to the conclusion that it resembles some of the remains of modern men dating from the Würm II period. Some archeologists, like Professor Skutil (1952, 1953), dispute its Pleistocene origin.

The calvaria from Podbaba near Prague, found in 1883, was also submitted to a detailed critical re-examination, since there were some doubts about its dating. Stratigraphic analysis of the age of the layer in which it was found, paleontological studies, examination of the site from the point of view of material culture, and the anthropological appraisal of the skull indicated that the find does indeed belong to the Pleistocene period and specifically to the so-called primitive forms of Pleistocene man (Vlček 1956).

Following Vlček (1952) and Jelínek (1958), the information based on the paleoanthropological finds in Czechoslovakia can be summarized as follows: The existence of Neanderthal man in the territory of Czechoslovakia is documented by two finds, made at Gánovce (Riss-Würm) and Šipka (Würm I). These represent the early, "generalized" form of Neanderthal man, and not the classical type. Man of the modern subspecies (*Homo sapiens fossilis*) is represented by several forms. The so-called primitive forms (represented by the finds Zlatý Kůň, Svatý Prokop, Předmostí, Brno II, Podbaba, and Ochoz) belong to the period Würm II. The finds from Dolní Věstonice, Brno III, and probably also from Pavlov, represent the more fully modern forms and belong geologically to the period Würm III. The placement of the finds from Mladeč is undergoing a re-examination; most likely these belong to Würm II. Chronologically most recent is the find Staré Město, belonging to the period Würm III. It represents a transition to Mesolithic man.

The extent and intensity of current archeological investigations raise justified hopes for further discoveries of Paleolithic skeletal materials. In this way the losses to Czechoslovak and world anthropology, caused in 1945 by the fire at the castle of Mikulov, will be at least partially compensated. It may not be inappropriate to re-emphasize the great service rendered to paleoanthropology by Professor Karel Absolon, the discoverer of Dolní Věstonice.

In the course of systematic explora-

tions carried on by the Archeological Institute of the Czechoslovak Academy of Sciences and the Central Geological Institute in Prague in different Karst regions of Czechoslovakia, primate remains which belong to the early phase of the Quaternary period were recently discovered. In the Zlatý Kůň cave in central Bohemia, with finds geologically dated from the second interglacial period ("Günz-Mindel"), and in the Gombasek cave near Plešivec (also "Günz-Mindel") in the Karst of southern Slovakia, bones of Cercopithecidae were found (Vlček 1959). The specification of the varieties has not yet been completed.

In spite of intensive work in paleoanthropology, prehistoric and historic anthropology has not been neglected in Czechoslovakia. Today, essentially all the skeletal materials obtained in connection with systematic archeological explorations and with incidental finds receive professional care in the anthropological divisions of the archeological institutes in Prague, Brno, and Nitra, and in some of the museums. Because of the limited space, it is not possible to cite here all publications concerning the description and anthropological analysis of literally dozens of finds. The list of authors is a long one and includes J. Chochol, J. Jelínek, E. Vlček, K. Hajniš, M. Stloukal, J. Pavelčík, and H. Malá, among others.

Substantial effort is devoted to the study of skeletal finds dating from the Mesolithic and Neolithic (Prošek and Vlček 1950, Vlček 1956, Hájek and Vlček 1956), Bronze (Jelínek 1950), La Tène (Stloukal 1959), Avar (Vlček 1956), and Slavic periods (Pavelčík 1949, 1955, 1959; Hajniš 1957, Stloukal in press). Vlček (1954) examined the Mongol remains found in Slovakia.

On the basis of numerous Old Slav finds, docent Fetter (1956) made an attempt to reconstruct their physique. He established that after they had left their common homeland north of the Carpathian Mountains, their physical features underwent substantial changes, particularly as the result of mixing with populations of different genetic backgrounds inhabiting the lands penetrated by the Slavs. The Slavs living in the territory of Czechoslovakia in the 10th century were of medium to high stature (165-170 cm), dolichocephalic, heavy bone construction, and well-developed musculature. Historical records from the early Medieval period indicate light pigmentation.

In recent years attention has been paid to the skeletal remains from cremation graves, and methods are being developed for the carrying out of an-

thropological studies of these specimens (Chochol 1953, 1955, 1956, 1958; Vlček 1956).

A number of anthropologists in Czechoslovakia are using the methods of Polish and Hungarian typologists (Czekanowski, Wanke, Michalski, Lipták, Nemeskéri) for the osteological study of racial types found among the prehistoric inhabitants of Czechoslovakia (Hajniš 1957, Malá 1957, Stloukal 1959).

A number of studies have been devoted to deformed (Lorenková 1956, 1958, Vlček 1952*d*, Lorenková, Pospíšil, Kelus 1957, Lorenková 1958) and trepanned skulls (Vlček 1953, 1956, Jelínek 1954, Chochol 1954, 1957, Hajniš 1956). The fact that the number of trepanned Old Slavic skulls found in the territory of Czechoslovakia has been increasing is of interest. Apparently trepanation was practiced only by the westernmost Slavs, inhabiting the territory of present Bohemia. It should be noted that the finds belong to the period of about the 10th century A.D., and not to the very early period of the arrival of the Slavs in the Czech lands. This fact, together with the lack of trepanation among other Slavs, suggests that the Western Slavs became acquainted with this practice only after their arrival in Central Europe, either from neighboring groups or from the older population inhabiting the territory later occupied by the Western Slavs.

Increased attention is being devoted to the study of diseases on the basis of the paleopathological findings on bones and teeth (Hněvkovský 1959, Pavlíková and Bílý 1959). The results of these studies have not yet been published systematically, but they promise to be of significance both for physical anthropology and for medicine.

An interesting observation was made by Andriš (1959) in his study of a substantial number of skulls dating from the 9th and 10th centuries A.D., obtained from various localities in southern Slovakia and southern Moravia. Maxillary anomalies, moderate in degree, were noted in only some 9% of the skulls examined. By contrast, in the contemporary population of Czechoslovakia the anomalies are found among 42% of the population examined, and are more marked.

Strouhal (1959) followed the occlusion relief of human molars on material dating from the last 3,500 years. He established that during this period there was a significant reduction in the number of protuberances in all the upper and the first lower molar teeth.

In this context, reference should be made to anthropological studies of exhumed skeletal remains of persons fa-

mous in the history of Bohemia, a field already diligently cultivated during the pre-war years. Docent Fetter (1951) described the skeletal characteristics of an outstanding Czech violinist of the 18th century, Ferdinand Laub. Vlček is engaged in the study of the physique of Vratislav I (died in 920) and Boleslav II (died in 999), the early princes of Bohemia whose tombs were recently discovered by I. Borkovský during archeological investigations of the area under the Prague castle.

ANTHROPOMETRY, WITH SPECIAL REFERENCE TO THE STUDY OF PHYSICAL GROWTH AND DEVELOPMENT

The aims of anthropometry were concisely stated by Prokopec and Kaplan (1958):

1. to provide objective criteria of the physique of individuals and groups;
2. to provide up-to-date norms and re-examine older normative data;
3. to characterize individuals and groups of individuals with reference to the total population;
4. to establish a base-line for the study of future trends;
5. to provide information on body dimensions (and, we may add, on other somatic characteristics, including body composition; cf. Brožek 1960) for the purposes of medicine, industry, and law.

In Czechoslovakia, anthropometry is closely related to school hygiene, and much effort has been devoted to the study of the body dimensions of the growing child and the adolescent. In this context anthropometry provides an objective assessment of the effects of changed hygienic conditions on physical growth and development.

Toward the close of the Second World War and shortly thereafter, numerous studies were undertaken which were concerned with the unfavorable effects of the war on the physical status of the Czech people, especially manifest among the youth, and with the process of recovery. During the years 1945-1950, investigations of the somatic development of children were undertaken by professional personnel associated with the anthropological institutes, colleges of medicine, pedagogical research institutes in Prague and Brno, and by teachers themselves (Lukášová 1945, 1947; Šobová 1946; Juráček 1948; Horáčková 1950; Hečko 1950; Mastník and Malý 1952; Kukura and Pastoriová 1953; Vodička, Hladká and Meisner 1954; and others). Even though most of these studies were limited as regards both the number of individuals examined and the territory that was covered, the majority of them were carried out carefully and provide valuable documentation regarding the physical development of the Czech and Slovak youth

during the first 5 post-war years. The data constitute very valuable comparative material for further research.

Striking results came out of the extensive study undertaken in the years 1949 and 1950 by the Institute of Social Medicine in Prague (Prošek, Horáčková, Hrubcová, Malý, and Mastník 1952), in which the data on heights and weights were compared with figures obtained 55 years earlier (Matiegka 1895). Thus, for instance, 6-year-old children in 1950 were on the average 12 cm higher and 4.5 kg heavier than children of the same age in 1895. The differences increased with age. The 15-year-olds increased in height by 14 cm and in weight by 13 kg. The comparison of measurements obtained in 1895, 1922, 1941, 1947, and 1949 to 1950 indicate that half of the gains were made during the last 5 of these years. These findings demonstrate that physical development may be significantly affected over a relatively short period of time. The facts involved are complex, and the significance of individual factors cannot be reliably ascertained at present. The presence of altered (accelerated) growth rates is not limited to city children. This was shown by Hladká and Vodička (1954), who examined over 22,000 children in rural Bohemia and Moravia. The study provides information on the relative growth rates of boys and girls between 6 and 15 years of age.

The accelerated growth rate of Moravian children was registered by Bednář (1952), Votava (1952), Ptáček (1952), Vašík (1954), and Dokládál (1953). In Slovakia the same phenomenon was observed by Kukura and Pastoriová (1953). In general, the city children (Prague and Brno) are tallest and heaviest, followed by the Czech and Moravian rural youth; the Slovak children are shortest and lightest.

Improved physical status of the population of Bohemia and Moravia is reflected also in the weight of the newborn. Doskočil (1953) found that the newborn children in Prague are somewhat taller and significantly heavier than they were 25 years ago. A similar finding was made in Brno by Dokládál (1955), who also noted some increases in the head dimensions.

As a result of the marked changes in growth rates, all pre-war age-height-weight tables became inapplicable and an urgent need was felt for new norms. This need was acknowledged by the Ministry of Health, which in February 1951 established a special committee with the mandate to plan and organize an extensive, all-state anthropometric investigation of Czech and Slovak youth. Professor Borovanský was named the chairman of the committee.

There were 4 sections of the committee:

1. Anthropological section, with subdivisions for general physical anthropology (V. Fetter) and for pediatric anthropology (J. Valšík), concerned primarily with the problems of methodology.
2. Health section (V. Prošek and O. Hněvkovský), responsible for the utilization of the anthropometric data for medical purposes.
3. Statistical section (J. Novák), concerned with sampling and evaluation of the data, and
4. Organizational section (J. Láb), responsible for the actual operations.

The investigation was planned very carefully, and it represented the largest undertaking since Matiegka's classical study of 1895. The measurements, made in the fall of 1951, involved determinations of height and weight of children between the ages of 3 and 18 years. The work was carried out with the aid of school teachers carefully trained in the techniques of measurement. Statistical evaluation of this very extensive material was undertaken by the Mathematical Institute of the Czechoslovak Academy of Sciences in Prague and the Center for Health Statistics in Bratislava. Docent Fetter and Dr. Láb presented the material in tabular form as a valuable aid to pediatricians and school physicians evaluating physical development. The mean values exceed, throughout, the previously established norms. Since there are regional differences in physique, separate tables were prepared for Czech and Slovak boys and girls.

While adequate data on the growth of children between the ages of 3 and 18 thus became available, information on children under 3 years of age was still lacking. A. Sobová undertook the task to fill this gap. In co-operation with her co-workers she examined anthropometrically some 5,000 children in Prague and 2,500 children from the region of Pardubice in eastern Bohemia (Sobová 1955, 1956, 1958, 1959). The data were summarized in the form of readily usable tables. The author established that the rate of physical growth is already accelerated in the early months of life. Earlier data suggested that Czech children doubled their birth weight by the 6th month of life; Sobová's data indicate that children today double their weight when they are about 4 months old. The weight is tripled on the average by the 12th month.

The investigations noted above provide a reliable picture of the basic characteristics of the somatic development of Czech and Slovak youth during the major part of the post-natal growth period.

Subsequent anthropometric investigations became concerned with the

factors affecting growth and the reasons for deviations from the norm. Clearly, cumulation of somatometric data has only limited value in the study of human physique, important as it may be for the purposes of applied anthropology.

At this point two comments may be appropriate. First, body build (physique) cannot be adequately characterized on the basis of height and weight alone. We have emphasized elsewhere (Brožek 1956, 1960a) the need for taking into account the lateral dimensions of the bony framework in arriving at an individual's standard (reference) weight.

The second point is a good deal more important. Extensive animal studies demonstrate beyond any doubt that rates of growth and patterns of development can be modified by nutritional means. The point which we wish to emphasize here is that when animals are reared on different "planes of nutrition," not only are there changes in body size and body composition, but longevity is affected as well. Experiments carried on since 1917 indicate with surprising consistency that in a variety of animal species a high plane of nutrition during early life is not compatible—other things being equal—with a long life span. It was noted that a lower plane of nutrition, and the resulting retardation of early growth, is associated with a *prolongation* of the life span in protozoa, water and fruit flies, silkworms, rats, and mice, as well as in cattle. In regard to the latter it had been found that cows which were fed 88%, 100%, 115% of the Scandinavian standard feed allowance prior to the first calving, and received normal (100%) allowance thereafter, had an average life span of 87, 80, and 67 months. In view of this overwhelming evidence, the pride that mothers, pediatricians, and physical anthropologists derive from accelerated growth rates of children appears to call for a thorough re-examination. Since longevity is the result of many simultaneously-acting factors, the problem of appraising the significance of human growth rates is anything but simple. Nevertheless, there are few problems in human biology that call more urgently for a critical study than the relations between early growth rates, on the one hand, and adult morbidity and longevity on the other hand.

One of the problems that attracted the attention of Czech students of growth is the significance of the birth weight. Stloukalová (1959) showed that children with extreme birth weights tend to maintain their position in the

weight distribution throughout the first 3 years of life. Very light children remain relatively light, and heavy children tend to fall into the upper part of the weight distribution. V. Staninová and A. Sobová (1958) examined the physical development of premature children, children of multiple birth, and children born at term but of very light weight (less than 2,500 g), compared with the growth of a healthy normal control group. The premature children advance faster than children born of multiple birth having the same birth weight, while the very light children born at term show significant retardation in weight gain.

The problem of seasonal variations in the rate of growth was studied in Slovakia by Valšík (1958). A substantial sample of children of varied ages was measured at regular intervals during a period of 12 months. The growth rates of height and weight were uniform throughout the year. No seasonal oscillations could be detected. This finding contradicts the observations reported by some authors who found that greatest gains in height are made during summer, and those in weight during fall and winter.

Blecha, Fischer, and Frühbauerová (1953, 1954, 1958) followed the growth of head and chest circumference and their relationships during the first year of life. In contrast to some observations reported abroad, the head circumference in the majority of children 3 months old was larger than chest circumference. Even at the age of 6 months, the head circumference was larger than the chest circumference in some 10% to 20% of the children of both sexes.

Prokopec (1958) studied the relation between birth order and stature in children 6 to 18 years old. He noted that the first-born children tend to be tallest, the second-born being average in height, and the third-born and those who followed tending to be shorter. In addition to the basic anthropometric characteristics (height, weight, chest circumference), attention is being paid by some authors to other aspects of human physique. Dokládál (1958, 1959, 1960) emphasized the significance of cephalometry as a subsidiary technique for the over-all appraisal of physical development in children. He examined a large number of boys and girls from birth to 20 years of age and established growth norms for the basic head dimensions. Kubiček and Kubičková (1959) studied the growth of external dimensions of the pelvis in girls, and recommended pelvimetry as a part of the medical examination of growing girls,

facilitating early recognition of a retarded or faulty growth of the pelvis.

Řičař (1954) and Vodička (1954) examined the growth of different parts of the body (the length of the trunk, and that of the extremities and their parts). Their study also included data on such physiological characteristics as vital capacity, respiration rate, pulse rate, and blood count. Crhák (1954, 1959) studied the morphology and growth of the hand in Silesian children, while Drobný (1959) examined the proportions of fingers in Slovak children. Chot (1958) showed that today's children of a given age exceed the pre-war dynamic norms. Suchý (1958) is interested in the theoretical aspects of the accelerated growth rates and is developing new techniques for the presentation and analysis of the growth curves.

Valšík (1955) devoted attention to the shift in menarche in Czech and Slovak girls. The present average is 13 years and 5 months. This problem, among many others, had been studied by Božo Škerlj, the Slovenian anthropologist who was trained in Prague under Matiegka, and who is at present the head of the Institute of Anthropology at the University of Ljubljana. In his "General Anthropology" (Škerlj 1959), he has referred to Czech anthropological literature published during the period covered in this review.

Valšík (1953, 1955) has been interested in the relation between the season of the year and the most frequent appearance of menarche, but was unable to come to definite conclusions. He noted the presence of acceleration in the eruption of permanent teeth during recent years (Valšík 1952, 1956). He is also examining the relationship between tooth eruption and the ossification of carpal bones (1955, 1959).

A number of anthropometric investigations carried out in recent years have focused on new problems. These included the influence of recreation on physical development (Meissner and Vodička 1954); mechanisms of growth retardation in children suffering from different diseases (Horácková and Kuncová 1958), including mongoloid children (Horácková and Hrubcová 1957); the influence of the family milieu and various other social factors (Hrubcová 1958); changes in the shape of the thoracic cage induced by various internal diseases in children (Nováková 1958); incidence of flat feet in Czech youth (Šmirák 1958, 1960); and the somatic development of adolescents engaged in different types of occupational work (Čech and Drdková 1958, 1959).

Significant changes over a period of 60 years were noted in the somatic structure of the adult inhabitants of the Czech lands (Fetter, Titlbachová,

Troniček 1956). In 1955 the average height of young male Czech adults, aged 25 to 39 years, was 173.2 cm, that is, 3.6 cm greater than in 1894. The height of women was 160.5 cm, showing an increment of 2.7 cm. The average weight of men in 1955 was 74.8 kg (increment of 4.6 kg); of women, 63.8 kg (a decrement of 1.0 kg). It may be interesting to note that American actuarial studies indicate similar trends. The comparison of studies published in 1912 and 1959 (Society of Actuaries 1959)—though not free from the complicating problems of sampling, and of changes in the weight of clothes and the height of shoes, especially among women—suggests that at a given height there has been a tendency toward *increased* weight among men (with averages ranging from 1 to 8 lbs. at different ages and heights) and average weight *decrease* (up to 6 lbs.) among women.

The increase in weight and height is especially marked among the younger Czech men. Among the 20-year-olds in 1894, 76% were below 170 cm in height. By contrast, in 1955 74.8% exceeded 170 cm and only 25.2% were shorter.

ETHNIC ANTHROPOLOGY

Before the First World War, and during the period 1918 to 1938, a number of Czech anthropologists participated in investigations of populations in different parts of Europe and in other continents. In Czechoslovakia itself, studies were carried out on the somatic characteristics of several nationality groups, while the somatology and the racial composition of the Czech and Slovak population received relatively less attention. Since 1945 the trend has radically shifted, with various population subgroups among Czechs and Slovaks becoming the focus of anthropological investigations.

Studies on height and weight were reviewed in an earlier section. In regard to the pigmentation of eyes and hair, Fetter (1958) reported the following frequencies: Among adult Czech men the eye color was dark in 8.6%, light in 39.0%, and intermediate in 52.4%; among women the corresponding percentages were 12.4%, 38.7% and 48.9%, respectively. The hair color among men was dark in 20.9%, light in 12.4%, and intermediate in 66.6%; for women the values were 18.7%, 13.0% and 68.3%, respectively. The number of individuals with red hair is below 1%.

The pigmentation of the skin, eyes, and hair was determined by various authors in the course of numerous investigations (Pavelčík 1948, 1949, 1957; Dokládál 1954; Suchý 1956; Valšík

1957, 1958; Šobová 1958; Hrubcová 1957). Titlbachová (1950), Dokládál (1954), and Suchý (1958) examined age changes in the eye and hair color. In children the hair unquestionably becomes darker with advancing age. No such uniform and clear-cut trends seem to be present in eye color.

Poláček (1950, 1951) examined the relationship between stature and head dimensions. He showed that the generalization implying a positive correlation between stature and dolichocephaly is not valid for the population of Central Europe. Correlational analysis of the data indicates that with increasing stature the width of the head increases faster than its length, a finding reported in 1932 by Valšík on the basis of his measurements of Montenegrins in Yugoslavia; the height and the width of the face increase approximately equally; and, finally, as the body height increases, the nose becomes narrower. Dokládál (1953) arrived at similar conclusions on the basis of his studies of the inhabitants of the region of Hlučín in Silesia.

Using the cephalic index as a criterion, Dokládál (1958, 1959a, b) studied ontogenetic development on the basis of measurements made on over 6,000 individuals. In the fetal period the head is brachycephalic and does not change. By contrast, the shape of the head undergoes significant post-natal changes. These may be grouped into three periods. During the first 6 months there is a marked tendency toward brachycephalization. Subsequently this trend is reversed, and rapid dolichocephalization takes place from the 7th to the 18th month of life. The trend continues, at a much slower rate, up to the period of maturity.

Age changes in various somatometric characteristics during the period of maturity were investigated in forest workers by Prokopec (1958), who has also shown the effects of physical labor on man's physical development.

Investigations designed to study somatic characteristics of the inhabitants of Czechoslovakia were initiated and carried out primarily by research teams associated with the Anthropological Institutes in Prague, Brno, and Bratislava.

The Anthropological Institute at Prague undertook in 1947 and 1949 investigations of Silesian school children and young industrial workers (Fetter and Šobová 1949; the work is not yet published in full). In 1958 Fetter studied the population of the region of Chodsko, in southwestern Bohemia, noted for its rich folklore.

The Anthropological Institute in Brno organized field research in Silesia, in the region of Hlučín (Dokládál 1953,

1954, 1955; Pospíšil 1959), as well as investigations of the somatic characteristics of the Lusatian Sorbs settled in Varnsdorf in northern Bohemia (Crhák, Lorencová, and Pospíšil 1956; Lorencová 1959; Pospíšil 1959; Ambroz, Poláček, and Pospíšil 1959), and of Czech emigrants who after the War returned to their homeland from Bulgaria (Poláček 1952). Independent anthropological investigations were carried out in the territory of Moravian Slovakia by Pavelčík (1948, 1949, 1957) and in the neighborhood of Brumov in eastern Moravia (Valašsko) by Suchý (1956).

In Slovakia, systematic studies have been pursued since 1955 by the personnel of the Anthropological Institute in Bratislava. The results of this work have not yet been published.

This brief survey of the more significant recent anthropological investigations indicates that in the period under review important information was obtained on the somatic characteristics of the inhabitants of different parts of Czechoslovakia. These data will facilitate the solution of a variety of problems of ethnic anthropology, including ethnogenesis.

The published material does not provide a comprehensive picture of the physique of the Czechs and Slovaks, but it does enable us to establish some regional differences. Fetter (1957) pointed out the existence of somatic differences between Czechs and Slovaks. On the average, the Czechs are somewhat taller and heavier; there are also some differences in pigmentation.

Valuable data for regional comparisons were obtained in the course of all-state anthropological investigations of forest workers in 18 localities (Suchý 1958a, b, c; Prokopec 1958). The comparisons between this occupational group and groups differing in regard to their living conditions may be of still greater interest. The forest workers live in relative isolation, and in many localities they may be regarded as the last remnants of autochthonous population groups.

In regard to regional differences, it was shown that the inhabitants of northern Moravia and the indigenous population of Silesia differ from the population of the central regions of Bohemia and Moravia. They are of smaller stature and a more slender body build (Dokládál 1953). Similarly distinct are the inhabitants of some sections of eastern Moravia in the region of Valašsko (Suchý 1956).

For the sake of completeness we shall note investigations by Czech anthropologists on other nationality groups. Thus A. Weiss (1947, 1948), continuing the pre-war studies of Professor Suk in

Subcarpathian Russia, examined the Ruthenian group of Lemki. Various anthropologists studied the physical characteristics of Greek children living in Czechoslovakia (Drtinová 1950, Hudcová 1950, Jelínek 1950, Molík 1950, Ryšánek 1950, Lorencová 1950, Crhák 1959). Titlbachová (1954) studied Korean youths who were temporarily living in Czechoslovakia, and Vlček in 1958 examined the physical structure and the racial composition of a sample of the present population of Mongolia (not yet published).

The last work by the dean of Czech anthropologists, Professor V. Suk, written in English (1955), is devoted to the problem of "Races and Racism." It represents the culmination of a lifelong effort to provide scientific documentation for the uniform origin of contemporary mankind, and the consequent equality of human races. Professor Suk concludes as follows:

The summary of the teachings of anthropology, human anatomy, pathology and the science of behavior show plainly that all races of Man are monophyletic in origin; the racial discrimination has no scientific foundation.

In the spirit of the founders of Czechoslovak anthropology, the anthropologists of Czechoslovakia are anxious to contribute their share to a better understanding among nations, and welcome collaboration with anthropologists of all countries.

ATHLETIC ANTHROPOMETRY

Anthropometric investigations in the area of physical education and athletics have been concerned principally with the effects of intensive exercise on physical development and structure. Somatic characteristics, including some functional features, were studied in outstanding volleyball players (Sabat 1949), marathon runners (Hornof and Schmidt 1949), boxers, bicyclists, hockey players, canoeists, wrestlers, weight lifters, and football players (Novák 1950, 1952a, 1952b), volleyball players, and swimmers (Novotný 1958, 1959). One of the goals of these studies was the quantitative description of the various "sport types" conceived as body builds characteristic of outstanding athletes engaged in a particular sport.

These investigations resulted in some medically relevant observations concerning, for example, body posture. Increased thoracic kyphosis was noted among various athletic groups. A tendency toward excessive lumbar lordosis was present among ice-hockey and football players; toward scoliosis among boxers and canoeists. In several groups (boxers, and basketball and football

players) unequal height of the shoulders was the rule rather than an exception. These observations were brought to the attention of athletic coaches, who were encouraged to introduce supplementary physical activities with the aim of compensating for possible unfavorable effects of the principal sport.

Anthropometric investigations of 100 top-flight female athletes indicated that the body build of participants in certain types of sport activity, such as swimming or rowing, differs consistently from that of a control group of women who do not participate in sports. However, the differentiation for the most part is not as marked as in men. Several factors account for this sex difference, including the fact that men are engaged, on the whole, in more strenuous physical exercises. Morphologically, the more ample stores of subcutaneous fat among women tend to conceal the muscular relief (Novotný and Titlbachová 1957).

Somatometric changes associated with 8 weeks of intensive physical training were studied by Suchý (1954), who reported an average increase of 2.2% in circumferential measurements.

Studies of the effects of exercise on body development and body structure are of great significance, theoretical (for physical anthropology) as well as practical (for sport medicine). Of interest are both the acute effects of physical training on somatometric and functional characteristics, and the chronic effects of physical exercise and its impact on man's physique, health, and longevity. This concern with physical activity injects into theoretical physical anthropology the welcome element of dynamics. On the practical side, systematic anthropological studies importantly supplement clinical case studies, and provide data relevant for a sounder planning of athletic training programs and gymnastic activities.

We have pointed out elsewhere (Čapková and Brožek 1958) the urgent need for investigations of such fundamental problems as the consequences, if any, of athletic participation by girls for the development of the pelvis, asserted to be narrower in adult female athletes. It is clear that only longitudinal studies will help to separate the factors of initial selection from the real somatic effects of athletic activity.

PHYSIOLOGICAL ANTHROPOLOGY

It may be of interest to note that while the founders of modern Czech anthropology (Purkyně, Matiegka, Suk) were outstanding proponents of physiological research methods in anthropology, the approach has not met with much enthusiasm among the rank and

file. It may be pointed out, as an indication, that Matiegka's revolutionary paper outlining an anthropometric approach to the study of body composition (Matiegka 1921) is much better known abroad than in the author's homeland. Only in recent years Jana Pařízková has devoted systematic attention to problems of body composition, using both direct body measurements (including the evaluation of the stores of subcutaneous fat, assessed by means of skinfold calipers) and the estimation of total body fat on the basis of body density determinations. Pařízková examined the relations between physical activity and body composition. In her sample of 13- to 14-year-olds, the average heights and weights of normally active girls and of girls regularly engaged in gymnastic exercises showed very little difference. At the same time, the layer of subcutaneous fat was markedly thinner (mean for 10 sites equalling 9.0 mm) in the gymnasts than in the control group, which averaged about 12.3 mm. This fact can be used as a textbook illustration of the importance of determining body parameters other than height and weight. Together with cross-sectional (group) comparisons, Pařízková carried out longitudinal studies on the effects of change in the intensity of physical activity. When the training program was interrupted for 10 weeks, there was a weight gain and an increase in subcutaneous fat. After the resumption of gymnastic exercises, the subcutaneous fat was reduced without changes in body weight (Pařízková 1958, 1959).

Bena and Pařízková (1958) examined the relation of body composition to the somatotype. Marked sex differences in the development of subcutaneous fat were demonstrated by Pařízková (1960) in a group of 380 boys, 10 to 17 years of age, and 300 girls aged 10 to 16 years, in whom skinfolds were measured and totaled for 10 sites. In adolescent girls there is a marked tendency toward a lower body density and a greater total body fat. Anthropometric data were supplemented by body density determinations in studies of growth and physical activity (Pařízková 1959a), and of re-alimentation and weight reduction (Novotný and Pařízková 1960).

HUMAN CONSTITUTION

Constitutional research, regarded as a part and parcel of anthropological research on the living man, has been intensively cultivated by B. Krajník and S. Mentl.

In Krajník's interpretation, "constitution" is a multifaceted concept including the external body form and the size and form of the internal organs and their functions, including the

psycho-physiological functions. The emphasis is on the reactivity of the organism as a whole and the interrelations of the different facets of organ systems. Of special interest are the interrelations between the endocrine and the central nervous system, important aspects of which are manifested as behavior.

Professor Mentl has carried out follow-up studies on "robustness" and constitutional type in some 7,000 individuals studied in the out-patient cardiological polyclinic in Prague, and in 650 athletes. He was impressed by the significance of the hereditary factors determining the basic morphological, biochemical, and functional characteristics of the human organism. The inherited tonus and reactivity of the autonomic system determine, in part, the reaction of tissues to hormonal, other chemical, and nervous stimuli. While the anthropometric aspects of the body build tend to change with age, the constitutional (typological) characteristics of the organism do not appear to be similarly affected (Mentl 1958). In recent years two typological systems have made their weight felt in Czechoslovakia: the system of "constitutional psychology" developed by W. H. Sheldon; and the typological classification deriving its air of authority from I. P. Pavlov's neurophysiological studies.

Undoubtedly, the emphasis on a "multi-phasic" study of human organisms, including the reactivity of the nervous system and the function of the endocrines, is a welcome development. In the past, physical anthropology of the living man has suffered much too often as a result of its isolation from the problems of the etiology (including genetics, nutrition, exercise, and other factors) of individual differences in physique and their biomedical significance.

HUMAN GENETICS

In the post-war years little attention has been paid to this field. Professor Valšík (1952) described 2 generations of individuals born from the marriage of an Indian husband with a Czech wife. Extreme variability was noted among the metric characteristics.

A unique case of congenital micro-

cephaly was described by Dokládál (1958). The cranial capacity was 405 cm; the brain weight of the 56-year-old man was 294.5 g. It appears that this is the smallest brain size of an adult man on the record.

Hrubcová (1959) examined the hereditary transmission of eye color in a sample over 4,500 parents and children. The data confirm the observations that the light eye color is inherited recessively, and the dark dominantly.

SEROLOGICAL ANTHROPOLOGY

Some data on the relative frequency of blood groups of the OAB system among Czechs and Slovaks were obtained in the pre-war years. More recently, Stolžová-Sutorisová (1947) reported the following frequencies for the population of central Bohemia, especially of Prague: O = 37.8%, A = 41.5%, B = 14.1%, AB = 6.6%.

Further east the frequency of blood group B tends to increase. This tendency was noted by Dokládál (1953) on the basis of his investigations among the native inhabitants of Hlučín in Silesia. The results were confirmed on a larger body of material from the same area by Valníček (1954), who reported the following frequencies: O = 34.9%, A = 33.7%, B = 22.2%, AB = 9.4%. In Slovakia the frequency of the B group also tends to exceed 20%, a value considered characteristic of Eastern Europe.

In recent years the problem of blood groups, with special reference to anthropological considerations, was investigated by M. Kout in the Institute of Hematology and Blood Transfusion, associated with the Ministry of Health, in Prague. In addition to the OAB system, Kout (1958) investigated, on a large body of material, the relative representation in the Czech population of blood groups characterized in terms of other systems. These data appear to us significant enough to be summarized in Table I.

FORENSIC ANTHROPOLOGY

Traditionally there has been a close collaboration in Czechoslovakia between anthropology, on the one hand, and criminalistics and forensic medi-

TABLE I

System	Frequency Data			
OAB	O, 31.35%	A, 45.18%	B, 16.70%	AB, 6.77%
MN	M, 33.54%	MN, 49.30%	N, 17.16%	
S	S positive, 53.68%		S negative, 46.32%	
P	P positive, 78.26%		P negative, 21.74%	
Rh	Rh positive, 84.15%		Rh negative, 15.85%	
Kell	Kell positive, 9.81%		Kell negative, 90.19%	
Duffy	Duffy positive, 62.66%		Duffy negative, 37.34%	
Secretary Capacity	Secretors, 77.13%		Non-Secretors, 22.87%	

cine, on the other hand. Anthropologists frequently serve as court consultants, especially in regard to the identification of skeletal materials and of hair. For purposes of forensic practice, V. Fetter (1953) prepared directives for the evaluation of skeletal remains and S. Titlbachová (1954) developed similar instructions concerning human hair. Several anthropologists (V. Fetter, L. Malá, B. Sekla, A. Šobová, J. Valšík) function as court experts in paternity disputes. Professor Valšík (1949) systematically analyzed the Essen-Möller-Geyer method of testing paternity, and pointed out some of its inadequacies. On the other hand, L. Malá (1955) presented interesting case studies demonstrating the validity of biological examinations of heredity.

DERMATOLOGICAL ANTHROPOLOGY

(pigmentation, dactyloscopy, dermatoglyphics)

Data on the color of the skin and the form and color of the hair are of basic significance for the study of human races. Significant work in this field was discussed in the section on ethnic anthropology.

Several investigators are concerned with the distribution of hair, including the extent of the hair coverage of the human body during childhood (Drobná-Krůtčlová 1958) and in adult individuals (Brückner 1952); the hair limits on the head (Lorencová 1950); and the prevalence of hair whorls at the top of the head (Dokladal 1954, Lorencová 1958) and on the back (Drobná-Krůtčlová 1959).

Jan Evangelista Purkyně (1787-1869), outstanding Czech physiologist and medical investigator (cf. Brožek 1957), is generally recognized as a co-founder of scientific dactyloscopy. In view of this fact, it is distressing to realize that until 1945 only a minimum amount of information was available regarding the dactyloscopic and dermatoglyphic characteristics of Czechs and Slovaks. Most of the investigations were carried out during the period covered by this review, and were undertaken by younger research workers, mostly trained under Professor Valšík. The studies carried out on samples of the Czech population included papillary patterns on fingers (Holomek 1950, Dokladal 1952, Crhák 1958) and toes (Dokladal 1953, Pospíšil 1959), and palmar and plantar dermatoglyphics (Juráček 1947; Šobová 1947; Holomek 1948; Pospíšil 1957, 1959; Malá 1957). In Slovakia, dermatoglyphic studies have been neglected, and investigations in this area were only recently initiated by M. Pospíšil. L. Malá (1957) has been studying the skin-pattern lines and whorls in reference to heredity. The remarkable phe-

nomenon involving the formation of papillary lines on the stump of the forearm after amputation was reported by Bartoš (1947).

Of international significance is the discovery of fingerprints of Upper Paleolithic man at Dolní Věstonice, found on lumps of clay. These are the oldest dermatoglyphics of human fingers known in the world literature. It is of interest that the pattern and the thickness of the papillary lines of fingers and palms of the hand are practically identical with the dermatoglyphs of contemporary man (Vlček 1952, Valšík 1952).

ANTHROPOLOGICAL METHODS

A manual for students, entitled "Anthropological Methods," was prepared jointly by M. Prokopec, J. Suchý, and S. Titlbachová. The mimeographed outlines, published so far in 2 editions (1953, 1958), contain a survey of the most important methods used in physical anthropology. In addition to the classical and generally applied methods, the authors described a number of new procedures, some of which proved to be useful additions to anthropological methodology. V. Fetter, J. Chochol, and S. Titlbachová (1955) developed an osteometric record-blank for the description of human skeletal remains. Their aim was to synthesize the procedures suggested and applied by various authors.

Professor Valšík (1959) examined the difference between the body length of cadavers measured in the horizontal and in the vertical position, in which they are held by special pincers. He found that bodies measured in the prone position were on the average 2.57 cm shorter. This observation is contrary to the usual experience on living man. Thus, E. Strouhal (1959) reported that the standing height was on the average 1.66 cm shorter than the body length determined in boys 11 to 16 years old when they were lying down.

M. Černý (1957, 1958, 1959a, b) is concerned with the estimation of stature on the basis of the dimensions of the long bones. In a reconstruction of average stature based on some 2,000 bones belonging to the historical inhabitants of Moravia (Hrádek near Mikulov), he applied the prediction equations developed by Manouvrier, Telkkä, and Trotter and Gleser, and compared the results obtained. The first procedure yielded the lowest stature, while the last of the three procedures gave the highest figures. In individual cases the differences between the stature esti-

mated according to these 3 procedures were distressingly large, with a maximum of 13.4 cm. These observations suggest the need to develop more precise estimation equations on the basis of data obtained on the population of Czechoslovakia. Such tables would be useful both for physical anthropology and for applications in criminalistic investigations.

The preparation of face masks using plaster of Paris has a long and outstanding tradition. Face masks of the living representatives of various nations of Europe, Africa, and America were obtained in large numbers by Czech anthropologists and explorers. They are kept in the Náprstek Ethnographic Museum in Prague and the Hrdlička Museum of Man. Prokopec (1951) described a new method for obtaining face masks of living individuals, and a procedure for obtaining several casts from one mold. In co-operation with the academic painter L. Lukešová, the staff of the Anthropological Institute in Prague (V. Fetter, M. Prokopec, and K. Hajniš) has undertaken the plastic reconstruction of man at different stages of his evolution, and of the Old Slavs (members of one of the Bohemian clans, that of the Slavniks).

TEACHING OF ANTHROPOLOGY

Anthropology is taught at all 4 principal universities, with the anthropological institutes located in Prague, Brno, Bratislava, and Olomouc. The students are drawn primarily from the ranks of future biology instructors who will teach in the upper grades of the middle schools (roughly corresponding to a combination of U.S. high schools and junior colleges) and in technical schools. The basic courses they take in human biology deal with somatology, i.e., anatomy and physiology of man. However, attention is also being paid to physical anthropology, including anthropometry and osteometry, the origin and development of man, interindividual differences (body "types"), and intergroup differences (the "races" of mankind).

At present, the staffing of the Institutes of Anthropology, the program of courses, and the laboratory facilities are adequate for specialized training in anthropology only in Prague and Bratislava. Academic work is planned for 5 years. During the first 3 years there is a common curriculum for all students of the biological sciences. The last 2 years are devoted to specialized studies. The students become acquainted with the principal areas of anthropology, and

with some of the neighboring disciplines with which anthropologists should be familiar, at least in regard to the basic facts and methods. These neighboring fields include archeology and ethnography, geology and paleontology, genetics and osteology, and statistical methods, with others as elective subjects. While the lecture and laboratory courses are taught in the main by the directors and assistants of the Anthropological Institutes, some courses are offered by personnel recruited from other institutes of the universities, or non-university research institutes and museums, and by applied anthropologists.

There are final state examinations in anthropology, and a thesis is required for graduation. The students who successfully complete the thesis and pass the state examinations receive the title of graduate biologists with specialization in anthropology. Each university graduates yearly some 3 to 5 students, who are placed in a variety of research institutes, medical establishments, and archeological institutes and museums.

So far we have been concerned with anthropology as a field of university specialization. However, courses in anthropology are taken regularly by students specializing in other areas. Some of them may be enrolled in colleges other than a college of natural sciences or biology. Thus, courses in anthropology are required for students of archeology and ethnography, subjects which are taught in the colleges of philosophy. In some colleges of medicine, e.g. Brno, elective courses are offered which introduce medical students to physical anthropology.

A small number of graduates pursue postgraduate studies and have the title of "aspirants." The purpose of the postgraduate training ("aspirantura") is to prepare individuals for scientific work in the chosen field. Postgraduate studies can be carried out on a resident or non-resident basis. In the former case ("internal aspirantura"), the studies extend over about 3 years; for non-resident students ("external aspirantura"), the period of study is usually longer. The work is completed by examinations in the student's field of specialization, and his presentation of a major thesis based on original scientific work. When the "aspirant" successfully passes the examinations and defends his thesis, he earns the scientific degree of a "candidate" of sciences. This system, following the Soviet pattern, was introduced only recently. A provision was made that established investigators, who have demonstrated capacity for independent creative work, may apply for the title of candidate of science on the basis of a thesis, without having to undertake for-

mally the lengthy postgraduate training ("aspirantura"). By the beginning of 1960 there were altogether 7 individuals in Czechoslovakia who had earned the degree of candidate of biological sciences, with specialization in anthropology.

ANTHROPOLOGICAL ORGANIZATIONS, MEETINGS, AND CONFERENCES.

While scientific societies have a well established tradition in the majority of the natural and social sciences, until 1946 there was no scientific organization uniting specialists in anthropology. Some attempts in this direction had been made before the Second World War, without bearing fruit. It is true that the number of professionally active anthropologists was quite small. However, in the neighboring European countries, in which anthropologists were not numerous, scientific anthropological societies had been in existence for some time, and some of them were very active. As a rule, these societies drew their membership not only from among the specialists in physical anthropology but also from the ranks of investigators in the neighboring fields—ethnographers, folklorists, anthropogeographers, comparative linguists, geologists, sociologists, archeologists, physicians, explorers, and paleontologists interested in the Quaternary. The possibility of an effective collaboration of specialists in the various aspects of the study of man as a biological and as a social organism has been tested by time.

Even before 1939, Professor Suk had endeavored to found a society designed to unite anthropologically-oriented scientists in Czechoslovakia, but the war made a realization of these plans impossible. He renewed his efforts in this direction in 1945. In 1946 the preparatory committee met; the constitution of the society was worked out; and on May 26, 1946 an official permit was obtained for founding the Anthropological Society, with its seat in Brno, a city located at the geographic center of Czechoslovakia. The aim of the Society is to provide a common ground for scientists active in anthropology and the neighboring fields, to facilitate the exchange of information and experience, to encourage scholarly and investigative activities, and to provide a medium for the publication of the results obtained in the course of these activities. In addition to its strictly scientific purposes, the Society is designed to provide a meeting ground for friends of anthropology and for lay persons interested in the anthropological sciences. Organization of popular lectures and of field

trips should serve the purposes of spreading information and interest in anthropology. Along technical lines, the Society participates in the acquisition and safekeeping of anthropological materials and has the right to organize technical exhibits.

Professor V. Suk served as the first president of the Society, with Professor B. Horák as vice-president, and Professor J. Valšík as general secretary. In 1960 the Society had over 200 members, some residing abroad (including one of the authors of this communication—J.B.). Over the past 15 years the Society has been very active. Membership meetings are held at approximately monthly intervals, with presentations made by members and invited guests from within Czechoslovakia and from abroad. In the period from 1946 to 1960, 106 scientific meetings were held and 180 reports were presented. The Society took part in organizing scientific anthropological conferences and undertook a number of activities designed to acquaint the public with the sciences of man. The Society publishes a Bulletin (*Zpravodaj*; prior to 1952, *News-Zprávy*). Individual issues are limited in size, but the journal has played an important role by enabling Czech anthropologists, especially the younger generation, to see the results of their investigative work in print. So far, over 100 original communications have been published in the journal, in addition to news concerning the Society's activities, personnel notes, and abstracts. The journal is sent as exchange material to anthropological societies and institutions abroad. Since in Czechoslovakia the *Bulletin* was the only medium of communication in anthropology, it has served abroad as a source of information about developments in Czechoslovak anthropology. Professor J. Valšík edited the journal from 1946 to 1952, when the editorial responsibility was taken over by M. Doklálal. It is regrettable that, because of financial difficulties, the size of the journal had to be reduced drastically.

The Society also publishes the non-periodical *Acta (Rozpravy)*. So far, 7 issues have been published in this series, including a bibliography of Czechoslovak anthropology, compiled by M. Doklálal. At present the activities of the Society are directed by the executive committee consisting of Karel Žlábek, president; Jindřich Valšík, general secretary; Milan Doklálal, executive secretary and editor; and Jarolím Schäfer, treasurer. The headquarters of the Society are in Brno, Komenského nám. 2.

In 1955 a branch of the Anthropological Society was established in the second university city of Moravia, Olomouc. In principle, its activities are

similar to those of the Brno chapter. There are over 20 members at present. Professor Josef Skutil, Moravian archaeologist and anthropologist, served as the first president of the Olomouc branch, a post now held by docent Zrzavý, head of the Institute of Anatomy at Olomouc.

In 1955 two other regional anthropological organizations were established.

In Prague, the anthropologists formed an Anthropological Section within the framework of the Division of Natural Sciences, National Museum Society. The Anthropometric Circle was founded in Bratislava.

Scientific meetings of the Prague Anthropological Section are held regularly. This group was instrumental in organizing the first and second conferences of Czechoslovak anthropologists, arranged a memorial program on the occasion of the 90th anniversary of the birth of Aleš Hrdlička, and participated in an exhibit concerned with Hrdlička's life and work. The group has no medium of publication of its own; and reports about its activities are published in the Prague *Journal of the National Museum (Časopis Národního Muzea)*. Docent Vojtěch Fetter is the section's president; Jaroslav Suchý, its secretary.

The Anthropometric Circle in Bratislava provides a meeting ground for Slovak anthropologists, professional and amateur (teachers, physicians). The Slovak students of anthropology are especially interested in the anthropometry of school children, and one of the tasks of the Circle is to co-ordinate research in this area. The Circle is headed by Professor Jindřich Valšík.

The aforementioned organizations represent administratively independent units, even though they have cooperated with each other in the past. It is highly desirable to unite them formally into an all-state Czechoslovak Anthropological Society. Plans have been formulated for establishing such a society under sponsorship of the Czechoslovak Academy of Sciences. This Society will represent, in the international forum, the interests of all anthropologists active in Czechoslovakia.

The organization of the all-state conferences of Czechoslovak anthropologists provided a practical demonstration of the usefulness of co-operation among the regional groupings of anthropologists and other institutions with interest in this field.

Before 1945 no separate conferences or meetings of anthropologists were held in Czechoslovakia; the anthropologists met within the framework of the meetings of Slavic Natural Scientists and Physicians. Meetings attended

by biologists working in various fields were continued after the war. The most significant of these was the all-state work conference of Czechoslovak biologists, held in Liblice near Mělník in 1952. The interests and concerns of Czechoslovak morphologists were represented by Professor Ladislav Borovanský, the well-known Prague anatomist and anthropologist. He outlined the pressing research tasks in all 4 morphological sciences—atomy, histology, embryology, and anthropology. He commented that in the past the work of the anthropologists suffered because of the lack of co-ordination, failure to carry out systematic studies which would cover all parts of the territory of Czechoslovakia, excessive concern with picayune problems, and the lack of team work. He noted that basic research in physical anthropology must be continued, but that it will be necessary to develop a closer collaboration with medicine, since anthropological facts and methods are of importance in various clinical branches of medicine, including pediatrics, dentistry, and orthopedics. Physical anthropology finds application in occupational medicine, in research on the interaction between man and his work environment. Anthropometric investigations are important in studying health, nutritional status, and the general standard of living of the population. In the interest of the co-ordination of anthropological research undertaken on the territory of Czechoslovakia, Professor Borovanský recommended that all-state meetings of anthropologists be held at least once a year. The purpose of these meetings was to present and discuss the work accomplished, and the plans of the regional groups and of individual scientists for future work. Borovanský's words of wisdom were received as a challenge and as a guiding thought by the Czech and Slovak anthropologists, and have had impact both on the content of research and on organizational developments.

Under the auspices of the Anthropological Section of the National Museum Society in Prague, the first all-state work conference of Czech and Slovak anthropologists was held in September 1955 in Starý Smokovec. With the beautiful landscape of the High Tatras as the background, and under the guidance of V. Suk, some 30 individuals took part in the conference. They represented all institutions in which active work in anthropology was going on. The principal goal of the conference was providing a mechanism for an exchange of information regarding the ongoing work, and for reaching some understanding and agreement on future research. The reso-

lution formulated at the conference points out some of the major tasks facing Czechoslovak anthropologists, and specifies some of the steps required for a healthy and effective development of anthropological research in Czechoslovakia. Since the anthropological personnel of the universities is relatively small and carries a heavy burden of teaching, leaving little time for investigative work, a recommendation was made for establishing independent anthropological research institutes in the framework of the Czechoslovak and Slovak Academies of Science—a pattern followed in some of the other countries of Eastern Europe, such as Poland and Rumania. These institutes would have two divisions, one for research on contemporary man, and one for paleoanthropology. The distressing lack of periodical publication media in anthropology was noted, and it was recommended to renew the pre-war *Anthropologie* and to make it the central journal of Czechoslovak anthropologists.

The second work conference, held in April 1957 at Kokorín near Mělník, with the participation of 51 Czech and Slovak and 5 Polish anthropologists, was more technical in character. Twenty-seven reports were presented, grouped into 3 categories: (1) anthropogenesis, prehistoric and historic anthropology, (2) physical anthropology in the more restricted meaning of the term, and (3) ethnic anthropology. In regard to organizational matters, attention was focused on preparations for the second all-state anthropological investigations, planned for the year 1961 and scheduled to cover a segment of the population from birth to 60 years of age. A 16-man committee of specialists headed by docent Fetter was elected and charged with making the preparatory arrangements for this ambitious undertaking. Members of the Anthropological Section of the National Museum Society in Prague edited and published in mimeographed form the proceedings of the first two anthropological conferences, with résumés of the reports and discussions.

The next meeting of Czechoslovak anthropologists took place in September 1958 in Opava, the ancient cultural center of Czech Silesia. In all respects the conference showed remarkable progress and frequently it is referred to as the first annual meeting of the anthropologists of Czechoslovakia. There were close to 100 participants, among whom 20 came from abroad, representing 7 different European states. Sixty-eight reports were presented, covering practically all fields of physical anthro-

poloogy. Following the congress, the participants visited a number of outstanding anthropological sites in Moravia, such as Předměstí, Mladeč, Brno, Mikulčice, and Věstonice. Some of the visitors took part in the celebrations of the 90th anniversary of Hrdlička's birth, held in Humpolec. The proceedings of the congress were published by the Silesian Institute (Silesian Institute, Czechoslovak Academy of Sciences, Opava, Nádražní okruh 31, Czechoslovakia).

A year later, in September 1959, the anthropological conference was held at Smolenice in Slovakia, in quarters made available by the Slovak Academy of Sciences. The conference was organized by the anthropologists of Bratislava, and it successfully matched the Opava congress in both the number of participants and the scientific level of the papers that were presented. As indicated by the substantial number (close to 30) of participants from abroad, the Czechoslovak anthropological conferences attract specialists from the neighboring states of Eastern and Western Europe. The forthcoming anthropological conference is planned, with international participation, for September 1961, and is to be held near Brno. It will be focused on the methodological problems of types in anthropology and dactyloscopy.

In addition to the special conferences, anthropological topics are represented on the scientific program of other scientific societies. Thus, anthropologists studying skeletal material obtained in archeological excavations (J. Chochol, E. Vlček, J. Jelínek, M. Stloukal, and others) take part in the meetings and conferences of archeologists and present there the results of their work.

Questions related to the somatic development of children are regularly discussed at the meetings of pediatricians. Furthermore, in March 1959 the Pediatric section of J. E. Purkyně Czechoslovak Medical Association arranged a separate, all-state work conference which met in Hradec Králové and was devoted to the problems of child growth. Almost all anthropologists working in this field took part in the conference and presented a number of reports bringing out the significance of anthropological investigations for pediatricians. Docent Kapalín and Dr. Prokopec spoke on the dynamics of growth and development, Dr. Šobová compared different graphic methods for following the physical development of infants, and Dr. Dokládál reported on the significance of cranial dimensions in the appraisal of the overall physical development of children. Dr. Parízková described the importance of studies on body composition based

on caliper measurements of skinfold thickness and of body volume, determined by hydrostatic weighing and considered together with body mass in the form of body density (Mass/Volume). Professor Blažek developed the concept of child's somatotype, and Professor Prošek described the scientific and practical requirements of the somatometric investigations on the population of Czechoslovakia, scheduled for 1961. A number of pediatricians associated with clinical establishments, out-patient clinics, and research institutes, reported on the practical uses of the results of anthropometrical investigations.

Anthropology is regularly represented at the work conferences of Czechoslovak morphologists, organized by the Czechoslovak Anatomical Society. Problems of identification, approached from the anthropological point of view, are discussed at the meetings of the Czechoslovak specialists in forensic medicine.

In the post-war years, Czech and Slovak anthropologists limited their participation in congresses held abroad, and restricted their field work principally to the countries of Eastern Europe. In the last few years they have also taken part in several conferences held in Western Europe. A 4-man delegation was sent to the Congress of Anthropological and Ethnological Sciences held in Paris in the summer of 1960. As was noted earlier, in 1958 Dr. Vlček spent 3 months in Mongolia as physician of the Czechoslovak archeological expedition, and had the opportunity to carry on some investigative anthropological work. Czech and Slovak anthropologists are anxious to carry on the tradition established by A. Hrdlička, V. Suk, J. Malý, and others who have participated in co-operative anthropological investigations in undeveloped areas, and thus to contribute to the general fund of knowledge in ethnic anthropology.

PUBLICATIONS

The publication possibilities in anthropology are very limited at present. So far, it has been impossible to revive the journal *Anthropologie*, founded with the help of Aleš Hrdlička in 1923 and published quarterly until 1941. The journal earned for itself a good name both at home and abroad, a fact substantiated by numerous papers published in it by foreign authors.

The only periodical publication specifically devoted to anthropology is the *Bulletin (Zpravodaj)* of the Anthropological Society, published in Brno. Because of severe space restrictions, the journal can now do little more than

record the activities of the Anthropological Society and publish abstracts of the reports.

Anthropologists are forced to submit their papers to journals in the neighboring fields but this, as can be readily imagined, creates problems. Best off are the specialists in historic and prehistoric anthropology whose works appear in archeological journals: *Archeological Reviews (Rozhledy)*, *Anthropozoikum*, *Slovak Archeology, Archeological Monuments (Památky)*, and others. Papers on ethnic anthropology can be found in *Czechoslovak Ethnography*, and publications in forensic anthropology appear in the journals *Forensic Medicine (Soudní lékařství)* and *Criminalistics*.

Worst off are the physical anthropologists. If their work has relation to anatomy, their papers may be accepted by *Czechoslovak Morphology*. Studies related to medicine can be published, with difficulties, in the *Journal of Czech Physicians (Časopis lékařů českých)*, *Czechoslovak Pediatrics*, and *Czechoslovak Hygiene*. Anthropologists associated with the universities frequently publish the results of their work in the yearbooks or in non-periodical publications of their colleges. Similarly, anthropologists active in museums publish in the journals and yearbooks of their respective institutions. In some respects this may appear to offer plentiful and varied publication possibilities to physical anthropologists. Not infrequently, however, it happens that an anthropological study, especially if it is fairly extensive, will not fit into any of the publication media in which anthropologists are welcomed or are tolerated as guests, and its publication must be sought abroad.

The splintering of the publication media makes it very difficult even for anthropologists within Czechoslovakia to follow the scientific output of their professional colleagues. For anthropologists abroad this is an impossible task, as one of the present writers (J.B.) can vigorously testify. Some of the journals in which the anthropological reports (usually written in Czech and Slovak only) are published have very limited circulation and are of only local significance. Thus it can readily happen that a valuable study will remain totally unknown to the wider scientific public.

It is hoped that in the near future it will be possible to re-establish a central anthropological journal which will systematically index all publications in anthropology issued in Czechoslovakia. On account of the anthropologists' highly diversified activities and institutional affiliations, it would be a hopeless task to attempt to publish all

anthropological work in the pages of a single journal. Nevertheless, the central journal should serve as an effective link between the Czech and Slovak anthropologists and their colleagues abroad.

SUMMARY AND CONCLUSIONS

During the Second World War, Czechoslovak anthropology suffered heavy material and human losses. Teaching, research, and publishing came to a dead stop. After the liberation in 1945, recovery was rapid and thorough. Today there are some 3 dozen professional anthropologists. They are active in 4 independent anthropological institutes associated with the universities, in the framework of the Academy of Sciences, in colleges of medicine, pedagogical institutions, research institutes maintained by the Ministries of Education and of Health, and in the museums.

The work program of Czechoslovak anthropologists is broad and encompasses essentially all facets of physical anthropology. In some areas (such as paleoanthropology, historical anthropology, and the study of child growth and of the somatic characteristics of the Czech and Slovak people) the level of accomplishment is high. Intensive research is also being done in the anthropological study of sports and of group (racial) differences in physique. In recent years encouraging progress was manifest in the field of anthropological research bordering on physiology. The Czech and Slovak anthropologists have been diligent in making observations and measurements; less attention was devoted to problems of anthropological theory. Also, the area of human genetics deserves greater attention than it has received.

The Czech and Slovak anthropologists do not work in a vacuum. On the

contrary, their labors are closely articulated with those of specialists in other scientific disciplines, including archeology, ethnography, criminology, industrial design, and medicine.

In summary, anthropology in Czechoslovakia on the whole is modern in direction, and its program is in harmony with the contemporary international developments of this field. In Europe it is in the forefront of anthropological activities. It is regrettable that, as a consequence of inadequate publication possibilities, the work of Czech and Slovak anthropologists is little known abroad. Even some of the major studies did not receive the attention they merit. It was a task of this presentation to survey briefly—for the benefit of the world-wide professional audience—the organization, research program, and the over-all accomplishments of anthropologists in Czechoslovakia during the 15 post-war years.

BIBLIOGRAPHY

ABBREVIATIONS OF PERIODICALS MORE FREQUENTLY USED

AMM	<i>Acta Musei Moraviae</i> , Brno.
AR	<i>Archeologické rozhledy</i> (<i>Archaeological Reviews</i>), Praha.
AZ	<i>Anthropozoikum</i> , Praha.
CLC	<i>Časopis lékařů českých</i> (<i>The Journal of Czech Physicians</i>), Praha.
CsE	<i>Československá Ethnografie</i> (<i>Czechoslovak Ethnography</i>), Praha.
CsM	<i>Československá Morfologie</i> (<i>Czechoslovak Morphology</i>), Praha.
CsP	<i>Československá Pediatrie</i> (<i>Czechoslovak Pediatrics</i>), Praha.
PA	<i>Památky Archeologické</i> (<i>Archaeological Monuments</i>), Praha.
PR	<i>Paedologické rozhledy</i> (<i>Pedological Reviews</i>), Praha.
<i>Przegl. A.</i>	<i>Przegląd Antropologiczny</i> (<i>Polish Anthropological Review</i>), Poznań, Poland.
PSOK	<i>Přírodovědecký sborník Ostravského kraje</i> (<i>Acta verum naturalium districtus Ostraviensis</i>), Opava.
SA	<i>Slovenská Archeológia</i> (<i>Slovak Archaeology</i>), Bratislava.
<i>Sb. lěk.</i>	<i>Sborník lékařský</i> (<i>Medical Miscellany</i>), Praha.
SN	<i>Slovenský Návodpis</i> (<i>Slovak Ethnography</i>), Bratislava.

ZAS	<i>Zprávy Anthropologické Společnosti</i> , Zpravodaj Anthropologické Společnosti (<i>Bulletin of the Anthropological Society</i>), Brno.
<i>Acta Bratislava</i>	<i>Acta facultatis rerum naturalium Universitatis Comenianae</i> , Bratislava.
<i>Acta Olomouc</i>	<i>Acta Universitatis Palackianae</i> , Olomouc.
<i>Publ. FSU Brno</i>	<i>Publications de la Faculté de Sciences, Université de Brno</i> .
<i>Sb. Kokořín</i>	<i>Sborník anthropologické konference v Kokoříně</i> (<i>Proceedings of the Anthropological Conference in Kokořín</i>).
<i>Sb. Opava</i>	<i>Sborník anthropologického sjezdu v Opavě</i> (<i>Proceedings of the Anthropological Congress in Opava</i>).
<i>Sb. Smokovec</i>	<i>Sborník anthropologické konference ve Starém Smokovci</i> (<i>Proceedings of the Anthropological Conference in Starý Smokovec</i>).
<i>Summ. Smolenice</i>	<i>Summaries of the Anthropological Conference in Smolenice</i> .
OTHER ABBREVIATIONS USED	
Čs	Československý (Czechoslovak)
s.	summary
AMBROZ, C.	1953. The finds of prehistoric dog skulls in Czechoslovakia. <i>ZAS</i> 6:21-23 (in Czech).
—	1958. Tierknochen aus der Bronzezeit von Dvory nad Zitavou. <i>SA</i> 6:66-81 (in Slovak, German s.).

—	1959a. Tierreste aus der Bronzezeit in Gánovce. <i>SA</i> 7:47-70 (in Slovak, German s.).
—	1959b. Mittelalterliche Haustierzucht in Budmerice. <i>SN</i> 7:560-68. (in Slovak, German s.).
AMBROZ, C., K. BIČAN, and P. POLÁČEK.	1959. Lausitzer Serben IV. Die Korrelation der einzelnen körperlichen Merkmale. <i>Publ. FSU</i> No. 406:409-17. (in Czech, German s.).
ANDRÍK, P.	1958. Schädel, Gesicht und Gebiss einiger Zwillinge. <i>Sb. Opava</i> , pp. 20-21. (in Czech, German s.).
—	1959a. Einige mittelslowakische endogame Gemeinden im Lichte orthodontischer Anomalien. <i>Acta Bratislava</i> 3:289-93. (in German).
—	1959b. Ein Beitrag zum Problem der Beziehung zwischen der Gesichts- und Gebissbreite. <i>Acta Bratislava</i> 3:295-301. (in German).
—	1959c. Gebissanomalien der prähistorischen Bevölkerung. <i>Summ. Smolenice</i> . (in German).
BALABÁNOVÁ, K., Z. ŠĚSTÝ, J. VAŠÍČEK, and P. V. ZAVŘELOVÁ.	1955. Contribution to the varieties of the temporal bone in man. <i>ZAS</i> 8:1-4. (in Czech).
BARTOŠ, A.	1947. Les dermatoglyphes du moignon de l'extrémité qui travaille. <i>ZAS</i> 1:2:2-6. (in Czech, French s.).
BEDNÁŘ, O.	1952. Somatologie der Schüler der Gewerbeschule in Břeclav. <i>ZAS</i> 4:81-83. (in Czech, German s.).
BENA, E., and J. PAŘÍZKOVÁ.	1958. Eine Methode der Auswertung des Unterhautfettes in der Wachstumsperiode. <i>Sb. Opava</i> , p. 22. (in Czech, German s.).
BLECHA, J.	1958. The importance of growth records for pediatrics. <i>CsP</i> 13:295-96. (in Czech, English s.).
BLECHA, J., O. FISCHER, and O. FRUEHBAUFEROVÁ.	1954. Head and chest circumferences in the infant and the relationship between them. <i>Pediatrické listy</i> 9:34-40. (in Czech, English s.).

- BLECHA, J., and O. FRUETHBAUEROVÁ. 1954. Development of some growth features in infants. *Sborník FLA* 2:1-20. (in Czech, English s.).
- BROŽEK, J. 1956a. Somatometric studies in human morphology: In memoriam Henrici Mattiegka, Melnicensis. *CsM* 4:163-77. (in English).
- . 1956b. Body dimensions and the estimation of men's reference weight. *Science* 124:685.
- . 1957. Review of *Opera Omnia, Tomus VI*, by J. E. PURKYNĚ. *Isis* 48: 388-89.
- . 1958. Some explorations in applied physical anthropology. *ZAS* 11:1:2-4. (in English).
- . 1960a. The Measurement of Body Composition. In M. F. ASHLEY MONTAGU, *An Introduction to Physical Anthropology*, pp. 637-79. Springfield, Illinois: C. C. Thomas.
- . 1960b. Review of *Build and Blood Pressure Study*, by the Society of Actuaries. *Human Biology* 32:320-25.
- BRUECKNER, V. 1952. Contribution to the knowledge of the morphology of the hair limits in man. Unpublished thesis. Brno. (in Czech).
- COUNT, E. W. (Ed.) 1955. Dynamic Anthropometry. *Ann. N.Y. Acad. Sc.* 63, Art. 4.
- CRHÁK, L. 1958. Das daktyloskopische Bild der Troppauer Jugendlichen. *PSOK* 19: 244-57. (in Czech, German s.).
- . 1959a. The morphology of the palms of boys from Opava. *PSOK* 20:329-41. (in Czech, English s.).
- . 1959b. Morphologie der Fusssohlen griechischer Knaben und Mädchen. *Acta Bratislava* 3:327-37. (in Czech, German s.).
- CRHÁK, L., A. LORENCOVÁ, and M. POSPÍŠIL. 1956. Die Lausitzer Serben. *Publ. FSU Brno*, No. 371:1-44. (in Czech, German s.).
- ČAPKOVÁ, J., and J. BROŽEK. 1958. Review of: *Sports in the cultural pattern of the world*, by ERNST JOELI et al. *Human Biology* 380:161-63.
- ČECH, O., and S. DRÁKOVÁ. 1958. Research on the physical development of the Slovak working youth. *Sb. Opava*, pp. 39-52. (in Czech, English s.).
- CERNÝ, M. 1957. Zur Feststellung der Körpergröße nach den langen Knochen. *Sb. Kokořín*, pp. 19-23. (in Czech, German s.).
- . 1958. Zur Geschlechtsdiagnostik bei langen Knochen der Gliedmassen. *Sb. Opava*, pp. 53-57. (in Czech, German s.).
- . 1959a. Geschlechtsunterschiede am Caput des Humerus und Femur. *Summ. Smolenice*. (in German).
- . 1959b. Die Körperhöhe der historischen Einwohner aus Hrádek, Bezirk Mikulov. *Acta Bratislava* 3:215-30. (in German).
- DOKLÁDAL, M. 1952. The dactyloscopy of the Czech youth from the Brno district. *ZAS* 5:5-8. (in Czech).
- . 1953a. Contribution to the study of papillary formulas on the toes. *ZAS* 6:5-7. (in Czech).
- . 1953b. The question of right- and left-side symmetry of the digital dermatoglyphs of the hands and feet. *ZAS* 6: 19-20. (in Czech).
- . 1953c. Anthropometrie der Bevölkerung von Hlučín. *Publ. FSU Brno* No. 343:1-40. (in Czech, German s.).
- . 1953d. Anthropological research on the Old Hlučín inhabitants in Dolní Benešov. *ZAS* 6:11-12. (in Czech).
- . 1954. Pigmentation and Blutgruppen der Bevölkerung von Hlučín. *Publ. FSU Brno* No. 353:61-95. (in Czech, German s.).
- . 1955a. The morphology of the auricle of the inhabitants of Hlučín. *Materiály i Práce Antropologiczne*, No. 9, p. 65. Wrocław (Poland).
- . 1955b. Basic characteristics in the physical development of the newborn in Brno. *CsP* 10:114-20. (in Czech, English s.).
- . 1955c. *Czechoslovak anthropological bibliography 1945-1954*. 40 Pp. Ed. Anthropological Society, Brno. (in Czech).
- . 1958a. Growth of the main head dimensions from birth up to twenty years of age. *CsM* 6:103-15. (in Czech, English s.).
- . 1958b. The changes in the cephalic index during the first twenty years of life. *CsM* 6:207-20. (in Czech, English s.).
- . 1958c. Ein seltener Fall hochgradiger Mikrocephalie beim Erwachsenen. *Scripta medica* 31:85-114. (in German, English s.).
- . 1959a. Ueber die Alterationen der menschlichen Kopfform im Laufe der postfetalen ontogenetischen Entwicklung. *Bericht über die 6. Tagung d. Deutsch. Gesell. f. Anthropol.* Kiel. Pp. 159-163. (in German).
- . 1959b. Growth and shape of head in man during the fetal period. *Scripta medica* 32:25-45. (in Czech).
- . 1959c. Growth of the main head dimensions from birth to twenty years of age in Czechs. *Human Biology* 31:90-109.
- . 1960. Head dimensions as a supplement for the estimation of the physical development of children. *CsP* 15:448-61. (in Czech, English s.).
- DOSKOČIL, M. 1953. Weight and height of the Prague new-born in 1951/52. *CsM* 1:143-47. (in Czech, English s.).
- DROBNÁ-KRUTĚLOVÁ, M. 1958. Vorübergehende praepubertale Behaarung der Gliedmassen bei Kindern. *Sb. Opava*, p. 71. (in Czech, German s.).
- . Das Vorkommen des Rückenhaarwurzels beim Menschen. *Acta Bratislava* 3:303-09. (in Slovak, German s.).
- DROBNÝ, I. 1959. Die relative Fingerlänge bei Kindern der nördlichen Slowakei. *Acta Bratislava* 3:339-46. (in Slovak, German s.).
- DRTIŇOVÁ, J. 1950. Les pieds des enfants grecs. *ZAS* 3:13-18. (in Czech, French s.).
- FETTER, V. 1951. Anthropological examination of the skeletal remains of the great Czech violinist Ferdinand Laub. *ZAS* 4: 33-41. (in Czech, English s.).
- . 1953. The methods for the identification of skeletal finds of man. *Knižnice SNB Praha*. (in Czech).
- . 1956. "The face of the Old Slavs on the basis of anthropological examinations." In *Origin of Slavs*, pp. 197-215. (in Czech) Praha.
- . 1957. Ethnic differences in the population of Czechoslovakia on the basis of anthropological investigation. *CsE* 5: 217-31. (in Czech, English s.).
- . 1958. Anthropological characteristics of the population in Czechoslovakia. *Sb. Opava*, pp. 80-86. (in Czech, English s.).
- . 1958b. Pigmentation des yeux et des cheveux de la population adulte en Bohême. *CLC* 97:819-23. (in Czech, French s.).
- FETTER, V., S. TITLACHOVÁ, and CH. TRONÍČEK. 1956a. Anthropological investigation of the adult Czechoslovak population. *CLC* 95:717-21. (in Czech, English s.).
- . 1956b. The evolution of characteristics of adult population in Czech lands during the last 60 years and the basic anthropological norms. *Universitas Carolina, Biologia* 2:209-32. (in Czech, English s.).
- FILUKA, J., and Z. VLÁŠIN. 1953. Anatomical relations of the ulnar nerve in the elbow region with the view to the professional ulnar nerve paralysis. *Lékařské listy* (Brno) 8:185-91. (in Czech, English s.).
- HÁJK, L., and E. VIČEK. 1956. Die Skelettgräber von Předměčice P. I. 47:1-30. (in Czech, German s.).
- HAJNÍŠ, K. 1956. Deuxième réimpression de Liblice sur Gidlina du X^{ème} siècle. *AR* 8:93-95. (in Czech, French s.).
- . 1957. Beitrag zur anthropologischen Typologie der alten Slaven. *Sb. Kokořín*, pp. 32-41. (in Czech, German s.).
- . 1958. Ein Vergleich der Typologie der Schädel aus Staré Město in Mähren. *Sb. Opava*, pp. 101-04. (in Czech).
- HŘEKO, J. 1950. The height and weight norms of Slovak children. In *Vademecum of the Slovak physician*, pp. 148-49. Bratislava. (in Slovak).
- HŘÍR, J. 1957. Die Lokalisation der Foramina nutricia an den langen Knochen des erwachsenen Menschen. *CsM* 5:94-109. (in Czech, German s.).
- . 1958. Formunterschiede der Fingerglieder der einzelnen Strahlen des menschlichen Handskeletes. *CsM* 6:221-34. (in Czech, German s.).
- . 1958. 1960. Das Längenwachstum des langen Knochens. *CsM* 6:116-25; 8:290-305.
- HŘÍR, J., and V. NOVÁK. 1957. Die Lokalisation der Ernährungslöcher an den Mittelhandknochen, Mittelfussknochen und den Fingerknochen. *CsM* 5:151-66. (in Czech, German s.).
- Hladká, V. 1958. Verfolgung der körperlichen Entwicklung der Schuljugend. *Sb. Opava*, pp. 110-14. (in Czech, German s.).
- Hladká, V., and A. VODIČKA. 1954. Mean heights and weights of school children aged from 6 to 15. In *Collection of Somatometric studies*, pp. 5-73. (in Czech).
- HNĚVKOVSKÝ, O. 1959. Pathology of the vertebrae in the population from the 7-9th century in Mikulčice. *CLC* 98: 1396. (in Czech).
- HOLOMEK, A. 1948. Les dermatoglyphes palmaires en Moravie. *ZAS* 1:5:7-8; 1:6: 1-4. (in Czech, French s.).
- . 1950. Dactyloscopy of the population of Moravia. *ZAS* 3:77-79. (in Czech, English s.).
- HORÁČKOVÁ, M. 1950. Anthropometric investigation of school youth in the district of Sušice in January 1950. *Pediatrické listy* 5:361-65. (in Czech, English s.).
- HORÁČKOVÁ, M., and M. HRUBCOVÁ. 1957. Anthropometry of mongoloid children. *Sb. Kokořín*, pp. 42-50. (in Czech, English s.).
- HORÁČKOVÁ, M., and Z. KUSCOVÁ. 1958. The follow-up study of 13-15-year-old children, retarded in growth. *Sb. Opava*, pp. 115-21. (in Czech, English s.).

- HRNČIŘ, J., and P. LIŠONĚK. 1960. Somatometric data of the pupils from Píerov I. *Acta Olomouc* 20:73-90 (in Czech, English s.).
- HRUBCOVÁ, M. 1957. The comparison of the color of hair and eyes of the Silesian school children. *PSOK* 18:465-81 (in Czech, English s.).
- . 1958. How the family environment influences the somatic development of a child. *Sb. Opava*, pp. 122-29 (in Czech, English s.).
- . 1959. The following-up of pigmentation in families. *Summ. Smolenice*. (in Czech, German s.).
- HUBCOVÁ, O. 1950. Les lèvres des enfants en Grèce septentrionale. *ZAS* 3:31-35 (in Czech, French s.).
- CHOCIOI, J. 1953. Analyse anthropologique des sépultures lusatiennes à incinération de Hrušov. *AR* 5:97-602 (in Czech, French s.).
- . 1954. Trépanation slave de Moravany près Pardubice. *AR* 6:635-36 (in Czech, French s.).
- . 1956. Analyse anthropologique des os humains de sépultures à incinération. *Proceed. of the Conference of Czechoslovak Archaeologists* 1955:16-25 (in Czech, French s.).
- . 1957. Le squelette avec le crâne trépané de Knézeves. *AR* 9:713-14 (in Czech, French s.).
- . 1958. Bisherige Ergebnisse einer anthropologischen Analyse der Lausitzer Brandgräber in den Böhmisches Ländern. *PA* 49:559-82 (in Czech, German s.).
- CHOI, J. 1958. The growth of muscle strength in relation to age. *Sb. Opava*, pp. 136-47. (in Czech, English s.).
- JANÁČEK, V., and I. MALEC. 1953. The effect of the loss of teeth on the form and dimensions of mandible in man. *Scripta medica* (Brno) 26:125-47 (in Czech, English s.).
- JUŘINEK, J. 1950a. Contribution à la morphologie de la main des enfants grecs. *ZAS* 3:25-28 (in Czech, French s.).
- . 1950b. Report on the result of the anthropological research in Cezavy in the year 1950. *AMM* 35:1-16 (in Czech, English s.).
- . 1951. A contribution to the classification of the Moravian Upper-Paleolithic man. *AMM* 36:1-12 (in English).
- . 1952. Beitrag zur phylogenetischen Stellung der mitteleuropäischer Neandertalerfunde. *AMM* 37:249-60 (in Czech, German s.).
- . 1953. Der Homo-Fossilis Zähnefund in Dolní Věstonice. *AMM* 38:180-90 (in Czech, German s.).
- . 1954a. The fossil man of Dolní Věstonice III. *AZ* 3:37-92 (in Czech, English s.).
- . 1954b. Crâne trépané de Mikulov. *AR* 6:18-22 (in Czech, French s.).
- . 1954c. L'anthropologie en Moravie à l'âge du bronze. *AR* 6:633-34, 645-46 (in Czech, French s.).
- . 1956a. Homo sapiens fossilis from Staré Město near Uherské Hradiště. *AMM* 41:139-96 (in Czech, English s.).
- . 1956b. Der fossile Mensch Brünn II und seine Datierung. *Sb. Kokořín*, pp. 61-63 (in Czech, German s.).
- . 1957. La nouvelle datation de la découverte de l'Homme fossile de Brno II. *L'Anthropologie* 61:513-15 (in French).
- . 1958. Neue Funde und Datierungen des paläolithischen Menschen in der Tschechoslowakei. *Homo* 9:203-08 (in German).
- . 1960. Anthropologie der Bronzezeit in Mähren. *Anthropos* (Brno) 10. Pp. 191, tab. 55. (in German).
- JURÁŠEK, B. 1947. Les dermatoglyphes plantaires en Moravie. *ZAS* 1:3-6 (in Czech, French s.).
- . 1948. La hauteur et le poids des écoliers de Brno. *ZAS* 1:4:5-7 (in Czech, French s.).
- KAPALÍN, V., M. PROKOPEC, and V. PROŠEK. 1957. Methodik der Verfolgung des Wachstums der Schuljugend. *CsP* 12: 420-30 (in Czech, German s.).
- KLÍMA, B. 1950. Sépulture de femme des chasseurs de mammoths à Dolní Věstonice. *AR* 2:32-36 (in Czech, French s.).
- KOUT, M. 1958. Blood-groups of the Prague population. *Sb. Opava*, pp. 151-56 (in Czech, English s.).
- KRAJNÍK, B., and E. STROUHAL. 1958. The Research of body proportions and somatotypes in 11-year to 16-year-old boys. *Sb. Opava*, pp. 157-60 (in Czech, English s.).
- KŘÍŽEK, V. 1956a. Types of nephrolithiasis and colour of eyes. *Sb. lěk.* 58:222-24 (in Czech, English s.).
- . 1956b. Somatic differences in various types of nephrolithiasis. *Sb. lěk.* 58:215-21 (in Czech, English s.).
- KUBIČEK, K., and B. KUBIČKOVÁ. 1959. Die Anthropometrie der Schuljugend: Die Messung der äusseren Beckenausmasse. Die Prophylaxe der mangelhaften Entwicklung des Beckens und sein fehlerhaftes Wachstum beim Mädchen. *Zschrft. f. ärztliche Fortbildung* (Jena) 53:1240-42 (in German).
- KUKURA, J., and O. PASTORIOVÁ. 1953. Durchschnittsgewicht und -höhe der Kinder aus Bratislava. *Bratislavské lékařské listy* 33:1-5 (in Slovak, German s.).
- LIŠONĚK, P., and J. HRNČIŘ. 1960. Somatometric data on the pupils from Píerov. II. *Acta Olomouc* 20:91-104 (in Czech, English s.).
- LIŠONĚK, P., J. POHANKA, and J. ZRZAVÝ. 1958. A contribution to the ontogenesis of the septum humerale. *CsM* 6:132-42 (in Czech, English s.).
- LORENCOVÁ, A. 1950. On the limits of hair on the head of Greek children. *ZAS* 3:6-13 (in Czech, English s.).
- . 1956. Artificial deformations of skulls from Velatice. *ZAS* 9:4 (in Czech).
- . 1956. Pathologische und artifizielle Schädeldeformation aus der Völkerwanderungszeit. *Publ. FSU Brno*, No. 397: 371-87 (in Czech, German s.).
- . 1958a. Die Schädel von Nový Šaldorf aus der Zeit der Völkerwanderung. *Sb. Opava*, pp. 171-75 (in Czech, German s.).
- . 1958b. Beiträge zur Kenntnis des Kopfhairwirbels. *Publ. FSU Brno*, No. 397:355-70 (in Czech, German s.).
- . 1959. Lausitzer Serben III. Kephalmetrische Merkmale. *Publ. FSU Brno*, No. 406:371-407 (in Czech, German s.).
- LORENCOVÁ, A., M. POSPIŠIL, and L. KELUS. 1957. Ein neues Skeletgrab von Vacenovice mit künstlich deformiertem Schädel. *Gottwaldov*. 24 pp.
- LUKÁŠOVÁ, L. 1945. Einfluss des Krieges auf die Entwicklung der Dreizehnjährigen. *PR* 1:71-74 (in Czech, German s.).
- . 1947. War and after-war changes in weight and height of school children from Prague. *PR* 3:161-66 (in Czech, English s.).
- MALÁ, H. 1957. Discussion of the typology of Slavs. *Sb. Kokořín*, pp. 193-94 (in Czech).
- MALÁ, L. 1955. On a hereditary case of an unusual pattern of palmar dermatoglyphs as a proof of paternity tests. *Sb. Smokovec*, pp. 26-27 (in Czech).
- . 1957. Zur Frequenz der Fersenschleife bei Tschechen. *Sb. Kokořín* 1957, pp. 67-78 (in Czech, German s.).
- MASTNÍK, B., and V. MALÝ. 1952. Neue Tabellen von Höhen- und Gewichtsdurchschnitten der Schuljugend ausserhalb Prag. *Pediatrické listy* 7:133-34 (in Czech, German s.).
- MATEJKA, J. 1895. The growth, physical development and somatic characteristics of the youth of Prague. *Rozpravy České Akademie Praha* 1895/96 (in Czech).
- . 1921. The testing of physical efficiency. *Amer. J. Phys. Anthropol.* 4:223-30.
- MEISSNER, J. 1954. The growth of children of preschool age. In *Collection of somatometric studies*, pp. 237-48 (in Czech).
- MEISSNER, J., and A. VODIČKA. 1954. The influence of recreation on the physical development of youths. In *Collection of somatometrical studies*, pp. 249-82 (in Czech).
- MENTI, S. 1958. L'évolution de la robusticité corporelle et du type constitutionnel au cours de la vie chez les hommes et les femmes adultes. *Sb. Opava*, pp. 189-95 (in Czech, French s.).
- MOLÍK, K. 1950. Une contribution à la connaissance des dermatoglyphes des enfants grecs. *ZAS* 3:28-31 (in Czech, French s.).
- NOVÁK, Z. 1950. Biotypological investigation of Czechoslovak wrestlers. *Sokol* 70: 97-116 (in Czech).
- . 1952a. On the influence of bicycle racing on the human body. *Cyklistika* 4: 372-77 (in Czech).
- . 1952b. Somatic characteristics of ice-hockey players. *Lední hokej (Ice-hockey)* 4:16-18 (in Czech).
- . 1952c. Somatic features of some sportsmen. *Lékařský obzor* (Bratislava) 1:413-16 (in Slovak).
- NOVÁKOVÁ, M. 1958. Verfolgen der Formveränderungen des Brustkorbes bei Erkrankungen im Kindesalter. *Sb. Opava*, pp. 201-04 (in Czech, German s.).
- NOVOTNÝ, V. 1958. Étude somatométrique de volleyballeurs tchécoslovaques d'élite. *Sb. Opava*, pp. 206-12 (in Czech, French s.).
- . 1959. Anthropologische und physiologische Bemerkungen zur Untersuchung der Schwimmer. *Summ. Smolenice* (in Czech, German s.).
- NOVOTNÝ, V., and J. PAŘÍZKOVÁ. 1960. Altered share of total body fat during realimentation and weight reduction. *Cs. gastroenterologie a výživa (Czechoslovak Gastroenterology and Nutrition)* 14: 279-80 (in Czech, English s.).
- NOVOTNÝ, V., and S. TILBACHOVÁ. 1957. Examination anthropométrique des championnes sportives tchécoslovaques. *Sb. Kokořín* pp. 86-88 (in Czech, French s.).
- PAŘÍZKOVÁ, J. 1959a. The development of subcutaneous fat in adolescents and the

- effect of physical training and sport. *Physiologia Bohemoslovaca* 8:112-17 (in English).
- . 1959b. On so-called active body tissue. *Čs. fyziologie (Czechoslovak Physiology)* 8:1-11 (in Czech, English s.).
- . The determination of body density in youths by hydrostatic weighing. *Summ. Smolenice* (in Czech).
- PAVELČÍK, J. 1948. Bourg Nivnice (L'Anthropologie de la population). *ZAS* 1:4:3-5 (in Czech, French s.).
- . 1949a. La composition raciale des habitants à Bánov. *Vlastivědný věstník moravský (Brno)* 4:3-7 (in Czech, French s.).
- . Matériel des os trouvés dans les fouilles à Staré Město. en 1948. *ZAS* 2:24-31 (in Czech, French s.).
- . 1957. Anthropologie der Bevölkerung von Kopenice-Gegend in Mähren. *Zprávy Musea v Uherském Brodě* 1957, No. 3:1-6 (in Czech).
- . 1959. Das Knochenmaterial aus dem altmährischen Gräberfelde in Veletiny bei Uherský Brod. *Acta Bratislava* 3:237-46 (in Czech, German s.).
- PAVELČÍK, J., V. HRUBÝ, and V. HOCHMANOVÁ. 1955. Die grossmährische Kirche und Begräbnisstätte in Modrá bei Velehrad. *AMM* 40:42-126 (in Czech, German s.).
- PAVIKOVÁ, J., and B. BÍLÝ. 1959. Investigation of dental caries in a fossil denture from Blučina and Staré Město in Moravia. *Čs. stomatologie (Czechoslovak Stomatology)* 59:75-89 (in Czech, English s.).
- POKORNÝ, V. 1953. Varieties of the sphenoid bone in man. *ZAS* 6:12, 17-18 (in Czech).
- POLÁČEK, P. 1950. The brachycephalization of the population of Europe. *ZAS* 3:86-92 (in Czech).
- . 1951. The relation of body height to the dimensions of the head as a contribution to the problem of brachycephalization. *ZAS* 4:7-10, 72-73 (in Czech, English s.).
- POSPÍŠIL, M. 1957. Quantitative analysis of finger-prints of the inhabitants of Hlučín. *ZAS* 10:1:3-6 (in Czech).
- . 1958a. Das gegenseitige Verhältnis zwischen den Hauptlinienansläufern und dem Auftreten von interdigitalen Gebilden. *Sb. Opava*, 213 pp. (in Czech).
- . 1958b. Zusammenhang der Papillarenzahl P (Valšek) und des Hauptlinienindex (main-line index Cummins). *Sb. Opava*, 214 pp. (in Czech).
- POSPÍŠIL, M. 1959a. Dermatoglyphik der Einwohner von Hlučín. *Acta Bratislava* 3:361-83 (in Czech, German s.).
- . 1959b. Morphologie der Hand der Lausitzer Serben. *Acta Bratislava* 3:311-25 (in Czech, German s.).
- . 1959c. The relations between the ending of the main line A and carpal triradii. *Acta Bratislava* 3:347-60 (in Czech, English s.).
- . 1959d. Korrelation zwischen den Dermatoglyphen an Fingern und Zehen. *Summ. Smolenice*. (in Czech, German s.).
- PROKOPEC, M. 1951. A method of making facial casts and how to get several casts from one mould. *ZAS* 4:83-87 (in Czech, English s.).
- . 1952. Some data about the longitudinal study of the children's weights and lengths. *S. Opava*, pp. 215-18 (in Czech, English s.).
- . 1958. The influence of the birth order on the body development of Czech children. *Sb. Opava*, pp. 219-22 (in Czech, English s.).
- . 1959. Körperhöhe und Körpergewicht tschechischer Kinder. *Bericht über die 6. Tagung d. Deutsch. Gesell. f. Anthropol.* Kiel. pp. 164-70 (in German).
- . 1960. The growth of Prague children from birth to 18 months of age as compared with some large foreign cities. *CSP* 15:427-35 (in Czech, English s.).
- PROKOPEC, M., and V. KAPALÍN. 1958. Anthropometry and school hygiene. *Przegl. A.* 24:171-97 (in Czech).
- PROKOPEC, M., and J. SUCHÝ. 1958. Anthropometric der tschechoslowakischen Waldarbeiter (1950-1952). *Práce výzkumných ústavů lesnických (Proceedings of the forest research institutes)* 14:13-54 (in Czech, German s.).
- PROKOPEC, M., J. SUCHÝ, and S. TILBACHOVÁ. 1953, 1958. *Textbook of anthropological methods*. Praha, 1st Ed. 1953; 2nd Ed. 1958 (in Czech).
- PROŠEK, F., and E. VLČEK. 1950. Sepultures méolithiques à Obřívství. *AR* 2:40-42 (in Czech, French s.).
- PROŠEK, V. 1936. The influence of the economic depression on the physical development of children. *Anthropologie (Praha)* 14:1-76 (in Czech, English s.).
- PROŠEK, V., M. HORÁČKOVÁ, M. HRUBCOVÁ, V. MALÝ, and B. MASNÍK. 1952. The physical development of the youth as a measure of the life standard of the people. *CLC* 91:2-10 (in Czech).
- PLÁČEK, M. 1951. The body dimensions of the pupils from Brno schools. *ZAS* 4:87-90 (in Czech, English s.).
- RYŠÁNEK, J. 1950. Une contribution à la morphologie du pavillon de l'oreille. *ZAS* 3:19-22 (in Czech, French s.).
- ŘIČAŘ, J. 1954. Body proportions of the school children in Prague. In *Collection of somatometric studies*, pp. 75-143 (in Czech).
- SKUTIL, J. 1952, 1953. Contribution to the supposed Pleistocene age of the anthropological finds in the cave Svätý Prokop near Prague. *Čs. Kvas (The Kvas Czechoslovak)* 5:76-78; 6:68 (in Czech).
- SOCIETY OF ACTUARIES: *Body build and blood pressure*. Vol. 1. 1959. Chicago, Ill., U.S.A.
- STANINGOVÁ, V., and A. SOBOVÁ. 1958. Body development of a followed group of pre-matures and twins in the first year of life. *Sb. Opava*, pp. 239-41 (in Czech, English s.).
- STLOUKAL, M. 1959. Skelette aus der Latènezeit in Mähren. *Summ. Smolenice* (in Czech, German s.).
- STLOUKALOVÁ, M. 1959. Der Einfluss des Geburtsgewichtes auf die körperliche Entwicklung der Kinder in den ersten drei Lebensjahren. *Summ. Smolenice* (in Czech, German s.).
- STOLZOVÁ-SUTORISOVÁ, V. 1947. Le centre des donneurs de sang à Prague. *CLC* 86:26-27 (in Czech, French s.).
- STROUHAL, E. 1958. Beitrag zum Studium des Gebisses bei jungen Männern. *Sb. Opava*, pp. 242-43 (in Czech, German s.).
- . 1959a. Ueber den Unterschied zwischen der Körperlänge liegender und stehender 11-16 jähriger Jungen. *Acta Bratislava* 3:201-13 (in Czech, German s.).
- . 1959b. Ein Vergleich des Okklusionsreliefs der Molaren der Populationen aus der älteren Bronzezeit, der mittleren Burgperiode und der Gegenwart. *Summ. Smolenice* (in Czech, German s.).
- SUCHÝ, J. 1954. Metrical changes observed after brief athletic training. In *Collection of somatometric studies*, pp. 285-90 (in Czech).
- . 1956. Die Körperigenschaften der Bevölkerung der Umgebung von Brumov. *PSOK* 17:213-30 (in Czech, German s.).
- . 1958a. Long-term observations of the physical maturation of the school children. *Sbornik FSP Praha (The Yearbook of the Higher Pedagogical School)* 1:247-60 (in Czech, English s.).
- . 1958b. Pigmentation des yeux et des cheveux dans les différents régions de la Tchécoslovaquie. *Przegl. A.* 24:160-70 (in Czech, French s.).
- . 1958c. La contribution au changement des signes taxonomiques. *Sb. Opava*, pp. 254-59 (in Czech, French s.).
- . 1958d. Indices of regional differences in anthropological analysis of Czechoslovak people. *Časopis Národního musea v Praze (Journal of the National Museum, Prague)* 68:31-37 (in Czech, English s.).
- . 1959a. Die anthropologischen Erkenntnisse aus den regionalen Forschungen in der Tschechoslowakei. *Čs. ethnografie (Czechoslovak Ethnography)* 7:177-88 (in Czech, German s.).
- . 1959b. Der Schädeltypus der altägyptischen Bevölkerung aus dem Begräbnisfelde Deir el Medinah (XVIII Dynastie). *Bericht über 6 die. Tagung d. Deutsch. Gesell. f. Anthropol.* Kiel. Pp. 145-47 (in German).
- ŠUK, V. 1955. Races and Racism: Science and Art versus Pretension. *Acta Acad. Scient. Cechoslov. Basis Brunensis*, 27:157-92 (in English).
- SVOBODOVÁ, E. 1953. Varieties of the os occipitale in man. *ZAS* 6:13-15 (in Czech).
- ŠABAT, K. 1949. Biotypological characteristics of volley-ball players. *CLC* 88:514 (in Czech).
- ŠKALOUB, F. 1949. Tooth extraction in orthodontia as an anthropological question. *Čs. Stomatologie (Czechoslovak Stomatology)* 49:61-68 (in Czech, English s.).
- . 1958. Die physische Anthropologie als Grundlage für die Forschung in der orofazialen Orthopaedie. *Sb. Kokošín*, pp. 102-03 (in Czech, English s.).
- ŠKERJ, B. 1959. *General Anthropology*. (in Slovenian). Ljubljana.
- ŠKERJ, B., and J. BROŽEK. 1952. Jindřich Matiegka and the development of Czech physical anthropology. *Amer. Journ. Phys. Anthropol.* 10:515-19.
- ŠMÍRÁK, J. 1959. Beitrag zur Somatologie der Olmützer Schuljugend. *Sbornik FSP Olomouc (Yearbook of the High Pedagogical School Olomouc)* 7:111-24 (in Czech, English s.).
- . 1960a. Les débuts et les cours de la maturation chez la population industrielle de Silésie. *Przegl. A.* 26:35-56 (in Polish, French s.).
- . 1960b. Flat feet in school youth and the working reserves. *Acta Olomouc* 2:1-88 (in Czech, English s.).
- SOBOVÁ, A. 1946. Die Ergebnisse der anthropometrischen Forschung am Realgymnasium in Prag. *PR* 2:273-80 (in Czech, German s.).
- . 1947. The configurations of the palm and sole as an individual and unchangeable sign. *PR* 3:169-85 (in Czech, English s.).

- , 1948. Maturity of Silesian girls. *PR* 4: 19-23 (in Czech, English s.).
- , 1949. Anthropology of the Silesian girls' maturity. *PSOK* 10:155-160 (in Czech, English s.).
- , 1956. Weight and body length of Prague children up to the age of 3 years. *CsP* 11:341-48 (in Czech, English s.).
- , 1958a. Couleur des cheveux et des yeux chez les enfants âgés de 1 à 36 mois. *Przegl. A.* 24:118-29 (in Polish, French s.).
- , 1958b. A comparison of results obtained in single examinations and follow-up study of infants up to the age of 12 months. *Sb. Opava*, pp. 281-83 (in Czech, English s.).
- , 1959. Growth and development of children up to 3 years. *Materiály i Práce Antropologické*, No. 28, pp. 1-132. Wrocław (Poland). (in Czech, English s.).
- , 1960. Comparison of graphic methods for follow-up of child development up to 3 years of age. *CsP* 15:442-47 (in Czech).
- TIHLBACHOVÁ, S. 1950. The color of the hair and eyes according to age. *ZAS* 3:83-86 (in Czech, English s.).
- , 1954a. The use of biological methods in criminalistics: The human hair from the criminalistic point of view. *Knížnice pro kriminalistiku (Criminalistic Library)*, No. 3, pp. 1-16 (in Czech).
- , 1954b. Zur Anthropologie der ko-reaner Kinder. *Vesmír* 33:336 (in Czech).
- TRONÍČEK, CH. 1957. Ein Beitrag zur Frage des Vorkommens neandertaloider Merkmale bei Homo sapiens recens. *Sb. Kokořín*, pp. 115-16 (in Czech, English s.).
- , Die morphologische und physiognomische Form des Gesichtes. *Sb. Opava* 284-88 (in Czech, German s.).
- VALNÍČEK, S. 1953. Contribution to the knowledge of the distribution of blood groups in the Ostrava district. *ZAS* 6:23-24 (in Czech).
- VALŠÍK, J. A. 1949. Quelle est la valeur de la méthode d'Essen-Möller et Geyer pour l'examen critique de la paternité contestée? *ZAS* 2:40-45 (in Czech, French s.).
- , 1950. Un cas d'échec de la preuve dermatoglyphique d'une paternité contestée. *ZAS* 3:38-41 (in Czech, French s.).
- , 1951a. The progeny of a Hindu-Czech marriage. *ZAS* 4:56-64 (in Czech, English s.).
- , 1951b. Zur Interpretation der Abdrücke der Papillarlinien des Menschen aus Dolní Věstonice. *ZAS* 4:94-95 (in Czech, German s.).
- , 1953. Sexual maturity of the female adolescents from Brno. *ZAS* 6:29-31 (in Czech).
- , 1954. Physical development of the youth from Brno. *Lékařské listy (Brno)* 9:305-09 (in Czech).
- , 1955. The median age at menarche in girls in Brno in 1953. *Bratislavské lékařské listy* 35:598-603 (in Czech, English s.).
- , 1956. The order of dentition of permanent teeth in preschool children. *Bratislavské lékařské listy* 36:205-18 (in Czech, English s.).
- , 1957. Die Pigmentation der Regenbogenhaut bei Kindern in Horní Liptov. *Sb. Kokořín*, pp. 117-18 (in Czech, German s.).
- , 1958a. Einige Erkenntnisse über den Rutilismus bei den slowakischen Zigeunern. *Sb. Opava*, p. 289 (in Czech, German s.).
- , 1958b. Ueber den Einfluss der Jahreszeit auf die körperliche Entwicklung der Kinder in Horní Liptov. *Sb. Opava*, pp. 290-94 (in Czech, German s.).
- , 1959. Zur Frage der Körperlängen liegender und stehender Leichen. *Acta Bratislava* 3:195-99 (in German).
- VALŠÍK, J. A., S. DOLEŽEL, and J. BURYŠKA. 1955. Die Beziehungen der Karpalknochenossifikation zur Körpergröße, Körpergewicht und Dentition. *Biologie (Bratislava)* 10:333-45 (in Czech, German s.).
- VALŠÍK, J. A., Z. KLUST, O. GRUEMMOVÁ, M. BULČKOVÁ, and E. OLEJNÍČKOVÁ. 1952. The eruption of the permanent teeth as a sign of the improved standard of living of our schoolchildren. *Pediatrické listy* 7:343-45 (in Czech, English s.).
- VALŠÍK, J. A., and V. FERÁK. 1959. Ueber das Verhältnis der Eruption der ersten Dauerzähne zur Ossifikation der Handwurzel-Knochen. *Acta Bratislava* 3:281-88 (in German).
- VLČEK, E. 1949, 1950. The travertine endocast of a Neandertaloid skull of Gánovce in Slovakia. *AR* 1:156-61; *ZAS* 3:48-60 (in Czech, English s.).
- , 1951a. Pleistocene man from the cave Svätý Prokop. *AZ* 1:213-26 (in Czech, English s.).
- , 1951b. Die Abdrücke der Papillarlinien des jungdiluvialen Menschen von Dolní Věstonice. *ZAS* 4:90-93 (in Czech, German s.); *AR* 4:520, 524, 529-30, 1952 (in Czech, French s.).
- , 1952a. Restes de l'Homme pleistocène de la grotte au Zlatý Kůň. *AR* 4:209-13 (in Czech, French s.).
- , 1952b. List of the finds of pleistocene man in Czechoslovakia. *AZ* 2:205-24 (in Czech).
- , 1952c. New finds of the diluvial man in the Bohemian Karst. *Actes du Congrès Panafricain de Préhistoire, II^e Session, Alger 1952*, pp. 783-89 (in English).
- , 1952d. Empreintes papillaires d'un homme paléolithique. *L'Anthropologie (Paris)* 56:557-58.
- , 1952e. Crâne artificiellement déformé de Klučov. *AR* 4:129-30 (in Czech, French s.).
- , 1953a. Trépanations sur les têtes osseuses du X^{ème} siècle. *AR* 5:198-202 (in Czech, French s.).
- , 1953b. Trépanation de l'époque romaine à Nitranský Hrádok. *AR* 5:455-59 (in Czech, French s.).
- , 1953c. Ein neuer Neandertalerfund in der Slowakei. *SA* 1:5-132 (in Czech, German s.).
- , 1954. Mongols et Mongoloides en Slovaquie méridionale. *AR* 6:70-71, 80-84 (in Czech, French s.).
- , 1955. The fossil man of Gánovce, Czechoslovakia. *Journ. Royal Anthropol. Institute*, London 85:163-71.
- , 1956a. The calf of Pleistocene man from Podbaba. *AZ* 5:191-217 (in Czech, English s.).
- , 1956b. Early holocene skeleton burials from Obříství near Mělník. *AZ* 5:233-85 (in Czech, English s.).
- , 1956c. Contribution à l'analyse anthropologique des sépultures incinérées. *AR* 8:724-27 (in Czech, French s.).
- , 1956d. Trépanation double de la période de la Tène de Dvory-sur-Žitava. *AR* 8:365-70 (in Czech, French s.).
- , 1956e. Das anthropologische Material von Žitavská Toň (Awaren-Zeit). *SA* 4:132-54 (in Slovak, German s.).
- , 1957a. Beitrag der tschechoslowakischen Paläoanthropologie zum Studium des Neandertalers. *PA* 48:1-14 (in Czech, German s.).
- , 1957b. Nouvelles trouvailles d'ossements de l'Homme pleistocène au Zlatý Kůň près de Koněprusy en Bohême. *AR* 9:305-10 (in Czech, French s.).
- , 1957c. Der pleistozäne Mensch aus der Höhle Zlatý kůň bei Koněprusy. *AZ* 6:283-311 (in Czech, German s.).
- , 1957d. Ein Menschenzahn pleistozänen Alters von Silická Brezová. *AZ* 6:397-405 (in Czech, German s.).
- , 1957e. Anthropologisches Material aus der Zeit der Völkerwanderung aus der Slowakei. *SA* 5:402-34 (in Czech, German s.).
- , 1958a. Zusammenfassender Bericht über den Fundort Gánovce und die Reste des Neandertalers in der Zips. *Archeological Institute, Praha*. 81 p. (in German).
- , 1958b. L'Homme neandertal à Spiš. *Przegl. Antrop.* 24:138-58 (in Czech, French s.).
- , 1958c. Die Reste des Neandertalmenschen aus dem Gebiete der Tschechoslowakei. In *Hundert Jahre Neandertaler*, pp. 107-20. Utrecht.
- , 1959a. Old literary evidence for the existence of the "Snow Man" in Tibet and Mongolia. *Man* 59: 133-34.
- , 1959b. Funde von altquartären Affen aus dem Gebiet der Tschechoslowakei. *Summ. Smolenice* (in Czech, German s.).
- , 1960a. The ABO System of Blood Groups in Khalkha Mongols. *Man* 60:91.
- VLČEK, E., and V. PELIKÁN. 1956. Application of the fluorine test to the Neandertal skull from Gánovce. *Man* 56:141-42.
- VLČEK, E., and J. KUKLA. 1959. Die Hallstattzeitlichen Kultmasken aus Menschenschädeln aus der "Hraška" Höhle im südslovakischen Karst. *PA* 50:507-56 (in Czech, German s.).
- VODIČKA, A. (Ed.) 1954a. *Sborník somatometrických prací (Collection of somatometric studies)*, 294 p. Praha. (in Czech).
- , 1954b. Body dimensions of the Prague boys. In *Collection of somatometric studies*, pp. 145-236 (in Czech).
- VOTAVA, L. 1952. *Somatology of Brno students*. Unpublished thesis. (in Czech).
- WEISS, A. 1948. Physical anthropology of the Lemeks of the Carpatho-Ruthenian Highlands. *Summa dissert. facult. rerum natur. Univ. Carolinae* 1947-1948, pp. 77-81. Praha (in Czech, English s.).
- ZRZAVÝ, J., P. LIŠONĚK, and J. POHANKA. 1959. Remarques concernant l'ostéogénèse du système huméral. *Przegl. A.* 25:135-50 (in Czech, French s.).

THE VOICE OF THE AMERICAN INDIAN:

Report on the American Indian Chicago Conference

by Nancy Oestreich Lurie

During the week of June 13-20, 1961, the casual observer on the University of Chicago campus found it difficult to identify the people who were meeting in small seminars and large plenary sessions. Statistics on health, education, and welfare were intermingled with citations of treaties, executive orders and public laws, while ever and again terms were used from anthropology—"cultural identity," "ethnic integrity," "role expectations." The plenary discussions sometimes sounded like a national political convention in full swing as some exasperated delegate might rise and ask, "But what about the problems of the people I represent?" In the late hours of the night, however, the observer would have had no doubt that the dark, earnest people speaking in soft, unfamiliar accents and dressed in everything from conservative business suits to flamboyant styles of the far West were American Indians. The campus rang with the sound of tom-tom, pow-wow songs, and the soft thump of dancing feet.

The average American is familiar with Indians primarily as colorfully dressed performers at summer tourist attractions, but the typical tourist does not know that such economic considerations as the paying audience and showy trappings are secondary to the singing and dancing which are enjoyed and engaged in for their own sake whenever Indians get together. They are part of being an Indian. Most Americans are vaguely aware that somewhere away from everything there is an "Indian Problem." They are singularly ignorant of the nature and immediacy of the problem. Being an Indian—which in many ways is a matter of choice—brings Indians into opposition with values deeply ingrained in the dominant seg-



Photo by Alan Berger

Plenary session of AICC sitting in Mandel Hall at the University of Chicago, June 16, 1961

ment of the American public and with official government policies. The social, economic, and particularly the philosophical complexities of the resulting impasse was what brought Indians to Chicago to engage in intensive discussion.

The American Indian Chicago Conference then was not just another University gathering exceptional only in being enlivened during off hours by activities unusual to the setting. The occasion was unique and of historic significance. For the first time Indian people of many different tribes from all over the country and representing many different points of view came together to compare and to discuss their problems just among themselves. Through a

free exchange of Indian experiences, feelings, and views, they sought to find sources of agreement on which future Indian policy should be built.

The Conference came into being through three interrelated factors: the aggravated state of Indian discontent; the increasing concern and development of new skills on the part of scholars and scientists in resolving social problems; and timeliness in terms of present opportunities to effect changes desired by Indians.

Historical Background

By 1960 the Indian Problem was more critical than ever despite some 150 years of many well-intentioned legislative and administrative efforts devoted to Indians and the expenditure of untold millions of dollars. The reason for this state of affairs is now very simple, at least to the anthropologist; but even for us it is the deceiving simplicity of historic hindsight as the accumulation of knowledge and development of analytical methods contribute to our understanding. Basically, the *Indian Problem* has been confounded consistently with *Indians' Problems*. Because government administrators and benevolent Whites have long been in agreement with In-

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Mrs. Lurie has done ethnological field work among a number of North American Indian tribes in the U.S. and Canada. She has been called upon frequently by Indians' attorneys to engage in research and act as a consultant and expert witness in claims brought by Indian tribes before the United States Indian Claims Commission. She served as Assistant Coordinator to the American Indian Chicago Conference, described in the present article.

Her most recent publication is *Mountain Wolf Woman*, the biography of a Winnebago tribeswoman, published by the University of Michigan Press.

Photographs in this article are by Benjamin Bearskin.

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dians as to the specific problems—poverty, inadequate education, poor health, alcoholism, and a host of other adversities besetting Indians—they have barely grasped, if they recognize the fact at all, that their definition of the Indian Problem is not generally shared by Indians. For the "White Man,"¹ the very existence of Indians constitutes the Indian Problem. (This, of course, is not peculiar to the case of the Indians. Such a problem exists whenever one group chooses to place this construction on the existence of another group and feels compelled to take measures, mild or drastic, to deal with it in terms unacceptable to the people who constitute the problem. The United States also has the Negro Problem; many countries have had or still have the Colonial Problem; and Hitler had the Jewish Problem.) With but one brief exception, this semantic confusion lies deep in the whole history of Indian-White relations. From the beginning it was assumed that the Indians would disappear as a result of military campaigns. When armed

¹ To the Indian the non-Indian neighbor is a White Man, regardless of ethnic origin, and the term is used here in this specialized sense.

conflict ceased, the Indians were scattered on reservations where disease, unsanitary conditions, and nutritional deprivation almost completed their extinction. However, as the charitable responsibility of a benevolent government, measures were enacted to make existence at least bearable for the remaining and no longer dangerous Indians. These survivors were expected to hurry along the path of the white man as the only course open to them. Although this was believed to be a natural, desirable, and inevitable process, the government endeavored for many years to hasten it by repressing Indian culture, language, and native forms of leadership.

The beginning of the century saw a gradual but accelerating increase in the population of Indian communities. This was due to a combination of welfare assistance rendered by the government, minimal though it was, and the Indians' own efforts to survive as Indians, learning to use what they could from the surrounding culture in order to maintain life itself. This is an important fact because for a long time, and with added emphasis in the last decade, the reservation system has been pointed

to as the cause of Indian persistence or "the basis of the Indian Problem." Isolated from the general public, so the argument runs, Indians tend to maintain their numbers among themselves and perpetuate distinctive traditions. It is a specious argument. In the first place reservations are not that isolated. Because legislation of the late nineteenth century permitted piece-meal alienation of Indian lands making many reservations patchworks of White and Indian holdings, there has been sufficient physical propinquity for Indians to absorb cultural influences from white neighbors, store-keepers, missionaries, and government personnel. Massive changes actually have occurred along lines selected by Indians, primarily in material goods and new technical skills. Furthermore, Indians are free to leave the reservations and participate in the larger society where in actual fact they come and go at will. As a rule, they suffer less from discrimination and prejudice than any other minority group in the United States, and a large number could "pass" if they wished to do so. In the second place, many Indian communities located in small areas of their original homelands which never were reserva-

tions are as clearly *Indian* as any to be found on reservations. The fact of Indian persistence as Indians must therefore be attributed not to enforced isolation on reservations, but Indian preference.

As Indian population increased in distinctive Indian communities, the Indian Problem increased. At the same time, various legal devices resulted in a constant whittling away at the land base so that small resources became even more inadequate and Indians' specific problems of employment and welfare became exacerbated. The need to reappraise the Indian situation was apparent and the year 1928 saw the publication of a large, thoroughly documented volume entitled *The Problem of Indian Administration*, popularly known as the Meriam Survey. It represented two years of intensive study carried out by a private firm under government contract (Brookings Institution, 1928). Although Lewis Meriam and his associates were imbued with the prevailing philosophy that Indians would eventually bow to Progress and lose their identity in the general population, they recognized that since this could not be forced the Indians' own customs and traditions meanwhile served an important and necessary purpose.

The tolerant view of the Meriam Survey backed by impressive factual documentation of needs to be met and recommendations for meeting them was a harbinger of a brief but striking change of outlook in Indian administration. This phase came into being during the first term of Franklin D. Roosevelt when John Collier was appointed Indian Commissioner, an office he was to hold from 1933 to 1945. Collier embarked on a "New Deal" for Indians, utilizing many of the findings of the Meriam Survey but investing his policy, as embodied in the Indian Reorganization Act of 1934, with what was considered a radical philosophy. To Collier the Indians were not the Problem; if there was "a Problem" it lay in America's blindness to the beauty and wisdom of Indian traditions that could enrich the totality of American culture which itself had sprung from a diverse heritage. Solutions to specific problems were sought with this concept in mind. Critics on one side called Collier a romantic and argued that he went to ridiculous lengths to reestablish long dead traditions thereby hampering the progress of groups which had made great strides toward acculturation. Other critics shared Collier's basic philosophy but pointed out that the format he developed for self-government and economic improvement was too arbitrary and no more suited to some tribes than earlier programs designed to expunge Indian

distinctiveness. In any case, by 1934 Indians had seen such frequent shifts of programs that as they viewed the wreckage of half-finished, ill-conceived, and mis-administered projects, they resisted the Collier Administration as doubtless just another grandiose scheme to be abandoned with the appointment of a new Indian Commissioner. Nevertheless, the degree of failure is mainly attributable not to the philosophy or format or even to the "apathetic" Indians, but to the lack of financial and moral support granted Collier. Congress provided him too little opportunity to implement, expand, improve, and adapt his ideas. The wonder is not that Collier's program fell short of its goals, but that wherever it worked at all it worked so very well.

By 1950, the time-honored philosophy of the Indian as the Problem again was dominant in official thinking. In the last decade policy regressed to the 19th Century with startling speed, and with a vengeance. The earlier philosophy assumed that if the Indians' specific problems were taken care of, they would stop being Indians and the Indian Problem would be solved. Now however, the idea was to get rid of the Indians by dispersing them and relieving the government of all responsibility for their specific problems. All the favorite catchwords of the day trapped the unwary: "efficiency," "economy," "democracy," "integration," "individual enterprise," "freedom." The government was going to do battle against all the social evils besetting Indians: paternalism, waste of government money, excessive bureaucracy, and, above all, "America's concentration camps," the Indian reservations.

Indian administration now was designed to "get the government out of the Indian business." Outstanding in the new legislative measures were Public Law 280 and House Concurrent Resolution 108, both dated 1953 but reflecting several years of previous attitude and discussion. Public Law 280 transferred the Federal jurisdiction over law and order on Indian reservations to individual states. Only five states were involved but they had sizeable Indian populations. The Indians protested, accurately predicting not only that problems of law and order would be aggravated (because the states would be unwilling to assume the cost of their new responsibilities for Indians living on tax-free lands) but also that agitation would begin for taxation of Indian lands.

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visions regarding particular tribes. Only a handful of tribes have been terminated to date and termination proceedings have been generally halted. Difficult legal complications were entailed in terminating even tribes considered in the Meriam Survey of 1928 virtually ready for withdrawal of federal supervision. But it was Indian resistance that blocked further termination, by utilizing these legal complications.

Indian Anxieties

To the Indians, reservations are not "concentration camps" but *home*, not just a place to live but where one has one's very being. There is an ineffable, indescribable emotional attachment to the land. Land is not thought of as plots of real estate owned by individuals, though individual allotments have been made on many reservations. It is true that termination was not overtly designed to separate the Indians from their land; they can keep it as long as they pay the taxes on it. But the Indians realize that unless a tribe is exceptionally well organized, economically solvent and has the sympathy of the local state in establishing a tribal corporation and fending off unscrupulous speculators, the land will be divided on a per-capita basis. Most Indians are poor and before they can put their holdings to productive use, if such a possibility even exists, they will have lost the land through tax default. Certain types of valuable lands such as forests, mineral areas, and grazing lands cannot be exploited by individual operators so that dividing the lands means heavily capitalized interests will buy them up and reap benefits Indians were prohibited from enjoying.

Tribal enterprises have always been hampered by bureaucratic red-tape and lack of trained personnel among the Indians themselves. However, the Collier administration had laid a ground-work for effective techniques of economic improvement and promoted education of young Indians to serve the interests of their own people. From the Indians' point of view agitation for termination had a special irony. Not only was it the old land-grab in a new disguise but it came just at a time when many Indian tribes could confidently anticipate standing firmly on their own feet—and on their own lands.

Still, much more is involved than simple economics to make Indians bitter. Termination and other abrogations of treaties and agreements for such purposes as building dams which will flood reservation areas give evidence to Indians, and to many non-Indians, that in Indian affairs expediency easily overrides historic "guarantees."

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The commitment of government to termination and relocation has produced a complex of new difficulties for the Indians. Money for health facilities, and long term loans for tribal enterprises and resource development has been withheld since such activities imply community perpetuation. The already serious factionalism among Indians which had come about through competition for power within the administrative structure imposed upon

them was intensified and compounded. City Indians were pitted against those on reservations. If relocation meant abandonment of reservation life forever, what right had city Indians to the meager benefits of reservation life? City Indians, intending to return home to live feared their birthright would be taken from them by their own Councils. Furthermore, old inter-tribal suspicions and hostilities were aroused in terms of reservation Indians versus those groups which had never possessed reservations. It was feared that programs designed to provide Indians with specific help, but which threatened Indian lands, would not be opposed by landless Indians who had nothing to lose and everything to gain. On the other side, landless tribes, often maintaining their distinctive communities in the face of prejudice and hostility because they maintain Indian cultures sometimes feel antagonistic to reservation Indians, who have their own lands and many special services.

History of the AICC

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Thus, in the midst of their troubles, Indians were in some cases sustained by the knowledge that they had understanding allies. In August of 1960, optimism was stirred by the prospect of a change in the national Administration. Neither presidential candidate was committed to the current policy and both parties took explicit notice of the need for special attention to Indian affairs. The possibility of a sympathetic administration was, however, not enough; a sound new policy acceptable to Indians had to be developed.

Among those vitally concerned with the future of the American Indian was Sol Tax who with his students had been working for many years on techniques for effective community development. As the presidential election drew near and it appeared that the Indians might really stand a chance of a fair hearing from the government, he began considering specific ways of arriving at valid recommendations. He had developed a concept of community action empha-

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If a really workable program were to be developed, somehow all Indians would have to be involved so that ideas could be exchanged, points of agreement discovered, and satisfactory recommendations produced. Gradually, the idea began to take the form of an *all* Indian Conference based on prior exchanges of ideas, through local discussions and general mail distribution of information. Other formats might have been used, but given factors of time and available facilities, this was as feasible as any, though it would entail constant work to sustain and bring to completion.

There are several important principles which must be observed in such undertakings. First, there is the need for fundamental faith coupled with much patience that the people involved are better able to solve their own problems, given the opportunity, than anyone else. This faith is at least as tenable as the faith of those who believe that experts are better able to solve a group's problems and go so far as to decide what the problems are in the first place. The approach of Action Anthropology does not exclude the use of experts. In fact the reason that it is Action Anthropology and not just spontaneous community effort is that the situation is such that the group is unable to get the process of problem solving started itself although it wants to do so. Outside help may also be needed to keep the process running smoothly and supply information and services which the group cannot provide for itself. In the case of the American Indian Chicago Conference (AICC) the major problem was the matter of communication between the widely scattered groups of Indians who appeared to form a real community of opinion, at least in many common grievances, and who had no opportunity to express their views widely or learn what other Indians were thinking. Suggestions for a process of communication and the use of experts as a source of

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help and information must, however, have relevance to the group and not be foisted on them.

Tax was fully aware of all the criticisms and prophecies of doom which would be dredged up from the murky waters of Indian administration once a plan such as he envisioned was widely known among non-Indians. Indians are factional. Indians are suspicious. Indians are apathetic. Indians are unable to handle their own affairs. Indians are too varied culturally from tribe to tribe to hold any views in common. Indians are at the mercy of the few political Indians who railroad through only measures that they want. And, as everyone familiar with Indian affairs knows, Indians cannot be worked with in this way because this is not the way Indians are worked with!

The above, like all truisms, are valid if surrounding factors are held constant. Tax proposed to change the surrounding factors and see what would happen. His first task was to find financial support for the project. Second, he had somehow to sound out enough Indians to see if he should proceed along the lines he planned or even proceed at all. Third, he had to be sure that sources of help and expertise would be available as needed. He first asked the University of Chicago to help finance the project from a special grant for public policy conferences. He saw the University as a neutral base of operations and the place to hold the conference since Indians are understandably suspicious of any hints of governmental or other special interests. More money would be needed from other sources and by happy chance in the summer of 1960 the Emil Schwarzhaupt Foundation, devoted to study of problems of education and citizenship, asked Tax to consider a study of Indian problems and development of recommendations as a guide for work with Indians.

The writer agreed to serve as assistant coordinator; Robert Rietz, Director of the Chicago Indian Center, became associate coordinator; two graduate students in anthropology at the University, Albert Wahrhaftig and Joan Ablon, also agreed to assist. With this volunteer staff assured, Tax accepted the task, worked out plans, and asked for funds. His letter of November 10, 1960 to Dr. Carl Tjerendsen of the Schwarzhaupt Foundation remained the fundamental guide in all further efforts devoted to the Conference:

In 1928 the great Meriam Report (supported by the Brookings Institution) assessed the total situation of U.S. Indians and made policy recommendations which resulted in the 1933-1950 "New Deal for Indians." Since 1928 we have (1) much new experience, including the experiment of that New Deal itself as well as unprece-

ded university, church, and organization work with Indians and the organization of Indians themselves; (2) a great deal of new social science, community development, and human relations knowledge relevant to problems of American Indians which has come in the past 30 years; and (3) a world situation that urgently requires that we resolve our domestic "colonial" problems, and in ways that could provide patterns for constructive democratic action in similar situations all over the world.

Therefore, the time is ripe to put together what we know for the guidance of American Indians, government agencies, voluntary organizations, foundations, universities, and the general citizenry. The time indeed is particularly strategic because a new administration in Washington will appreciate new information on the basis of which to act. From this last point of view, what is to be done should be done this Spring and the development of the report itself should be educational and "public" to all leaders involved. The proposal, therefore, is not to undertake long research, but to crystallize what is already known and develop on the basis of it a general "policy paper," and to do this communally among leaders of all groups involved in Indian affairs in North America.

The conference in May would bring together for some days or a week both scholars and leaders of all groups involved—Indians, organizations interested in Indians, foundations, government agencies of states, and U.S. and Canadian governments—who would already have participated in developing the document and who would then agree upon its final form. This final conference would function also to interest the citizens of the country as a whole. I am hoping that the University of Chicago will be host to the conference and will provide some basic financial support for it. If not . . . other means of financing can and will be arranged.

The conference in May then will discuss, amend, and adopt a final report which (with its supporting appendices providing scholarly, factual, and historical materials) would become a guide for new action on the part of all concerned. The report meanwhile will be developed as a continent-wide enterprise among the scholars and interested leaders, by correspondence and in regional and local conversations and meetings, with its form and content determined as it develops.

At the NCAI meeting in Denver, there is also a meeting of representatives of universities interested in Indian education and research—which meeting I have been asked to chair. This is the essential time to begin the task of organizing the development of the report. Whether there should be a committee or whether (as I think might be simpler) those who participate become a "committee of a whole" can be decided in Denver where work can also actually begin.

The time schedule would then involve:

1. By January 1, 1961 a preliminary memorandum, perhaps with a first draft report, would go to all of the "participants" (scholars and Indians and other leaders) with a request for reactions, revisions, information, etc.
2. Simultaneously: (a) A series of meetings and conferences, based on reactions to the first report, through which a "final" draft could be developed; and (b) a pulling together of the relevant factual material needed for the eventual report.
3. By May 1st there would go to all a draft of the final report to be discussed and revised in the conference scheduled for the

end of May (or early June). At this time the participants will also be told about final practical arrangements for the conferences.

In order to accomplish all this, I shall need good professional personal assistance; I have a tentative promise of help from Dr. Nancy Lurie of the University of Michigan who could act as secretary (or assistant director) of the project. . . .

If you wish me to undertake this task, I would appreciate your telegraphing me as early as possible at the Cosmopolitan Hotel, Denver. Hours there will make a difference.

The invitation of the National Congress of American Indians mentioned above provided Tax with a national opportunity. He realized that the membership of the NCAI is relatively selective and that the organization is viewed with distrust by some groups of Indians who consider it too politically oriented and with too many members presently or at one time employed by the Indian Bureau. But it is by far the most representative multi-tribal organization of Indians; and with its experience with organizational problems could both begin the development of the conference and also advise on its feasibility. If NCAI would agree with enthusiasm to work with all Indians, he would feel confident about going ahead, once assured of adequate financial support. That this support was supplied is noted, and succeeding events are also recorded in a second letter to Tjerendsen of the Schwarzhaupt Foundation, dated November 26:

Everything is working out unbelievably well. Your telegram was in Denver on my arrival on Monday the 14th, and I got to work at once, since this gave reality to a plan that would otherwise have been a bubble. Also, I reported to Chicago your grant, and on Wednesday morning got a telegram from the University promising \$10,000 for the conference if held in Chicago.

My best inspiration in Denver was realization that the first "model" of a report should be developed by Indians; that the major task then would be to get Indians of the nation to discuss and develop it, and that the first task of the Convention was the task of the Indians. With this clear in my mind, it was easy to talk to the Indians in Denver; and they passed a resolution supporting the whole scheme and agreeing to provide a first draft of a report. Also, they took advantage of a telegram from President-Elect Kennedy to seek a meeting with him to ask him to come to the Convention. I cannot exaggerate the positive attitude permeating the NCAI. Helen Peterson (Executive Director, NCAI) and D'Arcy McNickle (scholar and writer long active in NCAI) agreed to stay in Denver-Boulder to develop a document; later I learned that Clarence Wesley (President, NCAI) called a meeting of S.W. Indians in Albuquerque to discuss it the weekend of December 2nd. All looks good there.

At meetings of the American Anthropological Association in Minneapolis from November 17-20, I explained the program to the Council of Fellows. Enthusiasm was immediate and great. When I said that people in different regions had to take

initiative to help Indians discuss proposals, 15 of the midwest anthropologists immediately organized for action. The Institute at South Dakota and Arizona State at Tempe are planning to call together Indian leaders. I also had opportunity to discuss strategy with people who know the other tribes—e.g. the Navaho—and have acted on advice.

By now plans are pretty well advanced. Nancy Lurie is at work (she and Rietz spent a day together here) in Ann Arbor. Karl Llewellyn [Professor of Jurisprudence in the University of Chicago Law School] has with excitement communicated this new development to the Commission on Indian Rights, etc., of the Fund for the Republic, and I think they will alter whatever reporting plans they have had. The University is of course cooperating fully; I had a meeting with all administrative officials and we set aside the new dormitory for Indians, near the Field House and Bartlett Field, etc. I have also had conversation with NBC-TV in New York, hoping they might give us a regular program during the next months, not only to educate the public but to give us a means of communicating with those who are involved in discussions over the country.

Nationwide Communication

The project was successfully launched and now the work began. So many different activities were carried on simultaneously in the following months, and so much substantive information began to appear showing the direction and content of Indian thinking that a brief article can present only a bare outline. The devices used to forward the work of the conference in most cases served several purposes which were: to assure Indians that it was to be a conference of *all* Indians, not of any special group of Indians, and certainly not of any non-Indians; correct misunderstandings as they might arise; communicate news and information of interest to Indians; keep Indians in constant and widespread touch with one another to exchange their ideas; take stock of developments at regular intervals and report them; and publicize the conference among non-Indians who would help Indians as needed. The chief means used were the printed word and face-to-face meetings.

Printed matter was sent at regular and frequent intervals including the model charter which started the discussion, progress reports, coordinator's letters and reports, and supplementary information of a scholarly nature. The model charter prepared by the NCAI with a covering letter from its President included a Credo, a section on historic background, a series of specific recommendations, and a suggested list of factual appendices. All of this was quickly printed and mailed with the following letter printed on a specially designed letterhead of the "American Indian Charter Convention."

To: All American Indians

This looks like the cover of a book, but

it is only a letter, a "cover letter" for what follows.

What follows is a statement by a few American Indians about what might be done to improve the situation of all Indians. Perhaps you and your community of Indians will agree with this statement just as it is. Maybe not. That is for you and all Indians to decide. If you can agree on some program, I think it will have good results for all Indians.

You are invited to come to a Convention at the University of Chicago from June 13-20, 1961. The first four days of this Convention will be a General Council of Indians, in which you will have the opportunity to work out a "charter" for the future of American Indians. In the last two days of the Convention this charter will be explained to representatives of local, state, and federal governments, and to private organizations interested in Indian welfare. We hope that the President of the United States can come to receive the charter on behalf of the people of the United States.

Speaking for the University of Chicago, I asked the NCAI to help. We thought that to start with there ought to be something worked out in writing—something to discuss and work on—something done by Indians themselves. The NCAI agreed, and here it is, exactly as it comes to me. As I understand it was agreed upon at a meeting in Albuquerque November 30-December 1 by

Clarence Wesley, President, NCAI
Georgeann Robinson, Recording Secretary, NCAI
John C. Rainer, Treasurer, NCAI
Helen L. Peterson, Executive Director, NCAI
Martha Burch Evenson, Gallup Area Vice President, NCAI
D'Arcy McNickle, Member of NCAI, and Director of AID
Alvin Warren, Indian Education, BIA, Albuquerque

They have done a wonderful job in getting something together for you to work with. We all owe them thanks.

You will notice at once that I have freely marked up the NCAI's draft, which I personally think is very good. They wanted me to do this just as they want all Indian people to feel free to raise questions and discuss all the basic issues involved. The draft is intended to be a starting point; the final charter will incorporate the best that can come from all of you. In other words as Clarence Wesley suggests in his letter this draft is not something on which you vote "Yes" or "No" but rather something to take apart and put together again with your own ideas. We are also sending you several other documents that may be useful. We hope to send more supplementary documents in the near future.

I am sending all of this material not only to Indian groups but also to others who might be willing to help Indians do this job. If you know somebody to whom I should send this packet, because he will help you, please tell me at once. The next step is up to you: Will you please return the enclosed postcard *at once*, so we know that you have received this, and so that we can get you some help if you wish it. We shall be in touch with you again in two or three weeks. Maybe we shall send you a questionnaire that will make it easier to get your views on what the Indian Charter ought to be.

The NCAI document deals with mainly

what the government ought to do. There are many private organizations—churches, colleges, etc.—that try to help Indians. The "Charter" ought to give direction to the efforts of all of us who seek to be helpful to Indians.

We shall be in touch with you again. We want to collect all of the facts and the opinions sent to us by Indians all over the country. In the second round we shall report to you what everybody else says, so you can continue your discussion in preparation for the General Council in June here in Chicago.

Please return the postcard as soon as you can; and good luck in your discussions.

Arguments, disagreements, and new suggestions which this model elicited from Indians were incorporated into a second version (no longer called a "charter") mailed out in May. The final version "Declaration of Indian Purpose" developed at the Conference itself, further refined ideas. The most controversial part of the charter concerned a drastic change in the Bureau of Indian Affairs. Figure 1 shows the progressive changes and final choice. The Credo underwent similar changes. It began as an expression of the belief that Indians can manage their own affairs, easily mistaken for the idea that Indians should progress themselves out of their "Indianness." In final form, the language strongly decries policies which have been harmful to Indians and stresses the right to a decent life while holding to Indian identity and concepts. The historical section damned termination, and the other legislation and administrative measures considered as specific sources of Indians' problems; it was not changed substantially from the first version to the last. Concrete recommendations were made in regard to a host of particular matters of health, education, and welfare in its broadest sense. Of great interest was the growing recognition, with each successive version of the charter, of the rights of *all* Indians: reservation, non-reservation, non-federally recognized, relocated, and terminated Indians. Besides the supplementary documents sent with this first mailing, others were sent out as time went by, and distributed at the conference itself.²

"Progress Reports" (Fig. 2) were used

² Among the supplementary materials which the Indians received at one time or another were Alexander Lesser's "Education and the Future of Tribalism in the United States," Walter Taylor's "The Kinzua Dam Controversy," Paula Verdet's "Summary of Research on Indians in Saint Louis and Chicago," Robert K. Thomas' "Population Trends in American Indian Communities," and many other papers and documents. In addition, the University of Chicago Press made available at reduced price to Indians William T. Hagan's *American Indians*.

In order that basic objectives may be re-stated and that action to accomplish these objectives may be continuous and may be pursued in a spirit of public dedication, it is proposed that a new organic Indian Act be adopted. This Act would be comparable in scope and purpose to the Indian Trade and Intercourse Act of June 30, 1834, the Act of the same date establishing the Bureau of Indian Affairs, and the Indian Reorganization Act of June 18, 1934, which recognized the inherent powers of Indian Tribes as self-governing units.

Should this act cover all tribes or only those which agree? Should it include tribes which have been terminated? Should all communities that they are Indians be included?

The legislation we propose would define the responsibilities of the United States toward the Indian people in terms of a positive national obligation to modify or remove the conditions, whatever they may be, which produce the poverty and the lack of social adjustment which dominated the findings of the Meriam Survey of more than thirty years ago and still prevail as the outstanding attributes of Indian life in 1960.

Do Indians have an obligation or this?

Specifically, the legislation would: (1) abandon the so-called termination policy of the last administration by revoking House Concurrent Resolution 108 of the 83rd Congress or substituting therefor a statement of renewed dedication to principles of mutual understanding and agreement between the United States and the Indian tribes.

OK to throw out "Termination"?

As a necessary educational experience, tribes need to handle more of their own affairs.

(2) Adopt as official policy the principle of broad educational process as the procedure best calculated to remove the disabilities which have prevented Indians from making full use of their resources.

(3) Since the Office of Commissioner of Indian Affairs has often been subordinated to political expediency and does not lend itself to the sustained, long range planning and management required to accomplish the basic objectives of national Indian policy, the office would be abolished and in its stead would be created an independent, national Indian Commission.

Has this hurt your tribal programs? Have they been changed too often?

(4) The details describing the powers and procedural formalities to be contained in the new Indian act can be more fully developed at a later time, but in general the provisions should include:

Is this idea of a government agency in its own policies — a good idea?

A. Modification of the exclusive legalistic authority of the federal government in the field of Indian affairs, in favor of a public agency with power to contract for services and to call upon agencies of government for assistance, as needed.

B. Enlargement of the contracting authority of the United States in obtaining services for Indians to permit the widest possible participation by competent private and government agencies. Contracts for such services would be based on planning by Indian groups and would be designed to accomplish the purposes of the Indian groups.

Should your tribe be able to make its own contracts in carrying out programs? This says that tribes could agree to change treaties should all members have a say, or only those on the reservation? Should a majority vote or should at least 3/4 be necessary to change treaties?

C. Faithful observance of trusteeship obligations resulting from treaties and statutory agreements, until alternative arrangements are arrived at through decisions freely entered into by the affected Indian groups.

D. The establishment of a National Indian Commission charged with responsibility for the protection and development of Indian resources and the encouragement of individual and group adaptation to the American community. This Commission shall consist of six members, one of whom shall be Chairman, all to be appointed by the President of the United States. One-half of the membership of the Commission shall be persons of Indian descent. In making the original appointments to this Commission, two members shall be appointed for a term of two years, two for a term of four years, and the remaining two, one of whom shall be the Chairman, for a term of six years. Thereafter, all appointments shall be for six years.

Is this better than having the Bureau of Indian Affairs? Is being "Indian descent" the important thing?

Operating responsibility shall be placed in a Director of Program, who will serve at the pleasure of the Commission. The Director of Program shall be qualified by training and experience to work with people and to respect cultural differences.

Staff members shall be selected on a basis of training and experience in community work. Provision for additional training and orientation of staff members shall be included in all operating plans.

B. A New Indian Act?

In order that basic objectives may be restated and that action to accomplish these objectives may be continuous and may be pursued in a spirit of public dedication, it is proposed that a new Indian Act be adopted to strengthen the principles of the Indian Reorganization Act and to accomplish other purposes. This Act would be comparable in scope and purpose to the Indian Reorganization Act of June 30, 1934, which recognized the inherent powers of Indian Tribes of Indian Affairs, and the Indian Reorganization Act of June 18, 1934, which recognized the inherent powers of Indian Tribes. The legislation we propose would redefine the responsibilities of the United States toward the Indian people in terms of a positive national obligation to modify or remove the conditions which produce the poverty and lack of social adjustment as these prevail in the outstanding attributes of Indian life today. Specifically, the legislation would:

- (1) Abandon the so-called termination policy of the last administration by revoking House Concurrent Resolution 108 of the 83rd Congress.
- (2) Adopt as official policy the principle of broad educational process as the procedure best calculated to remove the disabilities which have prevented Indians from making full use of their resources. Since the office of the Commissioner of Indian Affairs has often been subordinated to political expediency and does not lend itself to the sustained, long range planning and management required to accomplish the basic objective of national Indian policy. [Explanatory note to (2). The alternatives suggested below are intended to provide a method by which Indian purposes will be facilitated and the relationship between the Indian people and the government made more responsive to Indian needs. In correspondence with tribal officials and individuals and in the regional discussions it is apparent that there is almost universal dissatisfaction with the present organization of the Bureau of Indian Affairs and its field operations. The opposition to the area offices is particularly outspoken and is supported by numerous instances of administrative delays and the defeat of Indian programs. In recent years the office of the Commissioner has been less and less an advocate of the Indian people and as a consequence the Indians have not had the representation at the top level of government to which they are entitled and which they must have if their programs are to have full support.]
- (3) Abolish this office and in its stead create an independent, national Indian Commission, located in the Department of the Interior, or in the Department of Health, Education and Welfare, or establish it as a separate, independent Commission reporting to the President. The Commission, to consist of three, six or nine members, would be charged by the Congress of the United States with responsibility for the protection and development of Indian resources and the encouragement of individual and group adaptation to the American community. The members of the Commission would be appointed by the President from a panel of names selected by Indians. In making the original appointments to this Commission, one-third would be appointed for a term of two years, another third of the members for a term of four years, and the remaining third (one of whom shall be the Chairman for a term of six years. Thereafter, all appointments would be for six years.

Operating responsibility would be placed in a director who, whatever his title, would correspond to the present Commissioner of Indian Affairs. He would be appointed by the Commission, and serve at the pleasure of the Commission. He and all of his staff would be qualified by training and experience to work with, to understand, and to respect the cultural values of the Indian people.

Alternatively to (3). In the discussion held in some regions the thought was expressed that a six-man (or some other number) National Commission might result in a divided responsibility and prove to be administratively cumbersome. Accordingly, in one region it was recommended that a nine-man Advisory Committee be appointed by the Secretary of the Interior with one member appointed from each of the nine areas. On recommendations of the Indians living in those areas, the Advisory Committee would be directly responsible to the Secretary and would consist of not less than five Indians. The purpose of the Committee would be to act as a watchdog over Indian Bureau activities. Terms of members should be staggered to assure continuity of policy.

As a cautionary note: In considering these alternative proposals, as between a six-man (or other number) Commission created by an Act of Congress, the members to be appointed by the President, and an Advisory Committee authorized by the Secretary of the Interior and the members appointed by the Secretary, it should be borne in mind that the Advisory Committee created by the Board of Indian Commissioners which was soon abandoned, it should also be remembered that the Board of Indian Commissioners which was created by an Act of Congress in 1865 was also intended to have the powers granted to the Board were completely inadequate. If there is to be any change at the top level of administration, it should be authorized by an Act of Congress with provisions for self-enforcement. It should also be created to serve the interests of the Indian people rather than the convenience of the government.

FIG. 1. Shows the development of the section on legislative recommendations. At top is the draft for discussion prepared by the N.C.A.I. with marginal notes by the coordinator. Bottom left shows the draft presented to the legislative plenary of AICC, and at bottom right is the amended document as it appears in the Declaration of Indian Purpose

LEGISLATIVE AND REGULATORY PROPOSALS

In order that basic objectives may be restated and that action to accomplish these objectives may be continuous and may be pursued in a spirit of public dedication, it is proposed that recommendations be adopted to strengthen the principles of the Indian Reorganization Act and to accomplish other purposes. These recommendations would be comparable in scope and purpose to the Indian Trade and Intercourse Act of June 30, 1834, the Act of the same date establishing the Bureau of Indian Affairs, and the Indian Reorganization Act of June 18, 1934, which recognized the inherent powers of Indian Tribes.

The recommendations we propose would redefine the responsibilities of the United States toward the Indian people in terms of a positive national obligation to modify or remove the conditions which produce the poverty and lack of social adjustment as these prevail in the outstanding attributes of Indian life today. Specifically, the recommendations would:

- (1) Abandon the so-called termination policy of the last administration by revoking House Concurrent Resolution 108 of the 83rd Congress.
- (2) Adopt as official policy the principle of broad educational process as the procedure best calculated to remove the disabilities which have prevented Indians from making full use of their resources. It has been long recognized that one Commissioner cannot give the personal attention to all tribal matters which they deserve. He cannot meet all callers to his office, make necessary visits to the field, and give full attention to the review of tribal programs and supporting budget requests. In view of these conditions, we most urgently recommend that the present organization of the Bureau of Indian Affairs be reviewed and that certain principles be considered no matter what the organizational change might be.

The basic principle involves the desire on the part of Indians to participate in developing their own programs with help and guidance as needed and requested, from a local decentralized technical and administrative staff, preferably located conveniently to the people it serves. Also in recent years certain technical and professional people of Indian descent are becoming better qualified and available to work with and for their own people in determining their own programs and needs. The Indians as responsible individual citizens, as responsible tribal representatives, and as responsible Tribal Councils want to participate, want to contribute to their own personal and tribal improvements and want to cooperate with their Government on how best to solve the many problems in a business-like, efficient and economical manner as rapidly as possible.

- It is, therefore, recommended that:
1. Area offices be abolished and their authority be given to the agency superintendents.
 2. The position of reservation Superintendent be strengthened to permit broader exercise of responsibility and authority to act on significant and important matters of daily operations of Indian problems, preventing undue delays.
 3. Position qualifications require the employment of Superintendents with courage and determination, among other qualities, to help with local problems and be willing to make without further referral to higher levels, decisions commensurate with the delegated authorities.
 4. The Superintendent be charged with the responsibilities of cooperating with the local tribal governing bodies in developing the Federal Program and Budget for that particular tribe or reservation.

to circulate Indians' views. Entire letters were reprinted along with reports on local and regional meetings devoted to AICC. It was impossible to reprint all of the letters received by the coordinators, but every effort was made to provide regional coverage and expression of all shades of opinion, so that the views of all Indians who cared to express them could be given equal consideration in deliberations. A few examples are included here to indicate the range and substance of these communications.

WHITE SWAN, WASHINGTON, March 27, 1961. It appears that the National Congress of American Indians or their delegates, were only concerned with minor details on the Indian situation and neglected the major issues. Our LEAGUE is not concerned with the continuance of the Bureau of Indian Affairs, or adding administrative personnel, cost, and policies. But rather in the dissolution of this branch of the Federal Government. Our concern is, to refer our Indian people back to their original status of "tribal self-government" in the absence of

dominance from another, in the SPIRIT and manner the ETERNAL CREATOR provided for all mankind. Now the Act of June 2, 1924 (43 Stat. L. 253), was just another waste of time by Congress. A dictator nation may be able to declare an individual of another power a citizen of that government, so long as that dictator nation may have control of the person of that individual in the absence of war. To declare an individual a citizen of a particular government against his wishes is one thing, but whether the individual will or will not accept such citizenship is entirely another matter. Therefore, such declaration will produce no effect, no more than if this government would presume to make a RUSSIAN citizen of this government. American Indians are citizens, only in the sense, by virtue of their original occupancy and the treaties which provided the status of "sovereignty," hence, qualified for self-government, and not by virtue of any delegation of powers from the Federal government and the States within it. The Constitution of the United States never intended to presume to legislate over the affairs of the Indians, no more than it would presume to legislate over the affairs of a foreign power.

"From the earliest years of the Republic

the Indian Tribes have been recognized as distinct, independent, political communities, and as such, qualified to exercise powers of self-government, not by virtue of any delegation of powers from the Federal Government, but rather by reason of their tribal sovereignty." (CH. 7, P. 122-123, F.L.)

For over 100 years the Indian people have been treated as illiterates, while other nations have progressed with their own governments established and governed by their own people, in far less time than the Indians. Yet your convention is planning to promote more Indian policies and regulations and to continue to retain the Bureau of Indian Affairs. More Indian funds including that of the taxpayers, appropriated for the sole purpose to maintain the Indian service, and dissipated in one form or another, in order to keep the payroll in effect for the benefit of the few while the Indians go hungry. Some Indian Reservations have potential resources, some do not, yet he is encouraged to leave his Reservation and resources to some other part of the country unknown to him and there left to be stranded. Such has the Act or (Public Law 959) the so-called "Relocation Program" done to the Indians. Such a program enacted by Congress, is just another attempt at integration in another color. The other important factor in connection with these Indian resources, is the fact, that so many of these policies and regulations have been established, which has hampered the Indian from utilizing his resource potential.

ALBUQUERQUE, NEW MEXICO, April 5, 1961. The aim of the forthcoming "Indian Pow-Wow" as stated in the Albuquerque Journal (March 28, 1961) is so absurd and preposterous that it would hardly be expected to originate or receive impetus from a university such as the University of Chicago. To assume that the Indian and his culture are unchanged and unchangeable is to make a baseless assumption. Furthermore, any attempt to revivify and sanction an antiquated superannuated way of life is to impede his progress toward individual responsibility and self-improvement, the lack of which will inevitably keep him "at the bottom of our heap in health, education and economic well-being." He will improve himself in these areas only when he ceases to live in the past.

The Indian needs not "an Indian bill of rights to fit the oldest frontiersman of the new frontier" or any special charters, but more individual responsibility and encouragement to face the new frontiers like any good American citizen. Anything short of this will put the new frontiersman in an old frontier of no progress when all about him progress continues to march on.

Therefore, as a full blooded Indian who believes in progress, and who knows well the detriments of antiquity, let me urge you to formulate plans that will help the Indian keep up with a modern way of life rather than to put him in a situation that will require his catching up later.

SMITH RIVER, CALIFORNIA, April 12, 1961. Many rancherias in California are being terminated; a few have already been terminated and have received titles. Our rancheria is expected to be finished and receive titles within a year. This is both good and bad. There are a few people that I believe will not have trouble paying taxes, but there are many who have not been able to build a home yet for lack of funds. Some of these are veterans of World War II and the Korean conflict, but they are unable to use their veterans' rights to

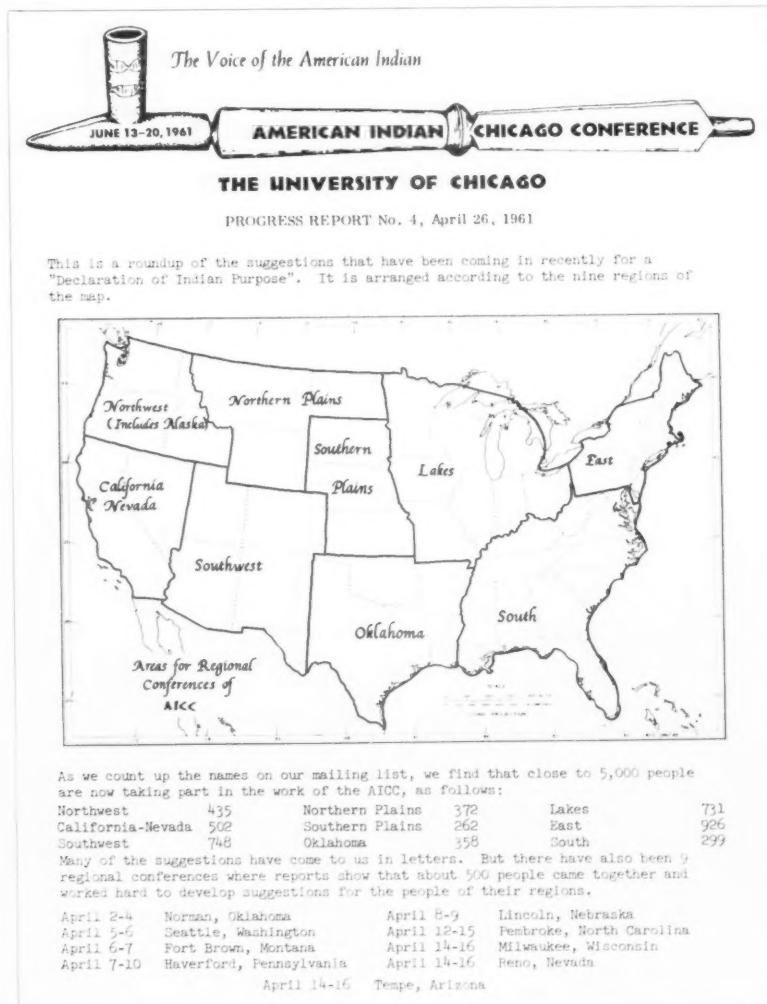


Fig. 2. Progress Report No. 4 giving dates and locations of regional meetings preparatory to AICC

get a loan to build, or get personal loans, because of not having their title.

I believe it is a crime on America that they discontinued reimbursable loans to individual Indians and that a program was never instituted to help Indians to manage their affairs. There are some that have received considerable money from heirship allotments and have squandered it. Now I ask if these people have title, where will they be but on welfare rolls or living off their relatives as long as they will support them. . . .

Most of our boys and girls never graduate from school. I believe the school system should be interviewed and get together should be held where some of these problems could be worked out. I would like to see more scholarships for Indian children. If there are any, we do not hear of them. I believe it would give our boys and girls more of an incentive. Even if a child is capable of going on to college, there are few that have funds.

One thing that I believe is very unjust against our people is that our Claims against the Government are not settled before they terminate us. Why can't termination be stopped until our claims are settled? Why can't a revolving loan fund be set up that is just for the Indians?

BROWNING, MONTANA, March 1961.
WE DISAPPROVE:

1. A Charter based upon the 1928 Meriam Report because its aims are too narrow.
2. An Indian Administration whose only function, according to the Meriam Report, is educating the Indian to assimilation and the use of the State and County services.
3. An "organic Act of the Sixties" to replace the Indian Reorganization Act. We don't want to give up the important guarantees of the present IRA.
4. A "Commission Plan" to replace the Indian Bureau. As Robert Yellowtail stated, "We don't want a six-man ball team over the Indians. We would never know who was carrying the ball."
5. A "Director of Program" to replace the Indian Commissioner.
6. A Point-4 Program independent of the existing Indian Bureau administration program.
7. A temporary "program-technician" at the Agency level who will "phase out," as the Point-4 people say it, and leave us high and dry without an Agency office.
8. An Indian Charter of the Sixties that will destroy our trust relationship with the Federal Government. This trusteeship began with the treaties and should remain.

9. A provision providing for the change of treaties. We don't intend to change them.

10. The limiting of community services on the Charter of the Sixties to those which will be made under *TRIBAL* contracts, with the local private, State and County agencies. Why should the Tribe pour all its development money down the drain for these services? Besides, our experience with these contracts since 1950 have been very unsatisfactory. They haven't done the job for the Indian people. There are too many restrictions. The people suffer.

11. An exchange of elected tribal councils for "selected local workers."

12. A Charter for the Sixties that would destroy our Federal charters of the Thirties.

WE WILL SUBSTITUTE FOR THE ABOVE:

1. A Charter of the Sixties based upon the Presidential statements of John F. Kennedy.
2. An Indian Administration which will be for *CONSERVATION AND DEVELOPMENT* in Indian Affairs on our own reservations, especially:- the conservation of the human and natural resources of the Indian tribes, the protection provided to America's first minority from exploiting forces *WITHIN* and *WITHOUT* the tribe. Only an Indian Commissioner can do this.
3. A Federal Administration that will supervise, coordinate, and promote the financing of services for Indians in health, welfare, law and order, and resource development.
4. A Charter that will promote a "Better Business Climate" on reservation for Indians.

FORT HALL, IDAHO, March 29, 1961. Through conformity with the Constitution and By-laws, the Law and Order Code and our Treaty, most tribal government could be successfully carried out here at Fort Hall, Idaho. However, it is always the case that the Bureau of Indian Affairs confronts us with a more recent regulation, code or what-have-you which supersedes our constitution, etc. Therefore, we are continually blocked in our efforts to conduct our tribal affairs. What we need from the Bureau is advice and technical assistance at a local level, rather than from someone sitting back in Portland with very little knowledge of the local situation. We have all the instructions, plans, and methods necessary to carry on our affairs in the

documents I have mentioned and they are sufficient in themselves for our needs if we were just allowed to follow them.

BLACK RIVER FALLS, WISCONSIN, March 27, 1961. In the past, much of the Federal Government's policy and programming for American Indians have been geared and directed for the benefit of those Indians residing on reservations or on non-taxable lands. Hence, many Indians have been excluded from receiving Federal aids and benefits.

We, the Wisconsin Winnebagoes, who do not reside on any reservation, and who, the greater number of us now reside on taxable lands and who need Federal assistance, have often been excluded from receiving Federal aids and benefits and have often been penalized in our efforts to help ourselves.

We are interested in a change of policy—a policy that includes aids and benefits for all Indians whether they live on reservations, non-taxable or taxable lands. The need for help is great in both areas. We want to include a policy change as more and more Indians will be going off reservations in the future.

INDIAN NECK, VIRGINIA, May 8, 1961.

The Voice of the Rappahannock Indian People, or Tribe

We have no schools to educate our children and we want schools that will educate our children, and train them to the highest education, such as lawyers, doctors, teachers, etc., and equal rights as any other citizens of America.

We have always been tax-paying citizens, and have never reaped any benefit. We are crowded out. The white neighbors hold the lands, and won't sell, and we need more area to take care of that situation.

Our people can get no jobs in our communities. They hire the white, and seldom can an Indian get anything to do. They have to go north or some other place in order to land a job. Therefore, we need some adjustment in order that we can live.

I would like a provision made for the old aged who are unable to work, that they can be cared for.

This is the voice of my people.

Chief
Rappahannock Indian Tribe

Significantly, the two groups which rejected the whole idea of the conference were the "assimilationists," and the

Regional meeting in the Milwaukee (Wisconsin) Public Museum, April 14-16, 1961





Left: meeting of the Steering Committee in International House on the University campus, April, 1961. Below: members of the preliminary committee meet with the coordinating staff and a member of the President's Task Force on Indian Affairs, February, 1961



FIG. 3. Report of the Coordinator dated March 16, 1961. Name of AICC was changed from "Charter Convention" to "Chicago Conference" at Indians' suggestion

extreme traditionalists. Actually, only one assimilationist took the trouble to write and the letter was published to indicate that an Indian could feel that the only solution to Indians' problems were through complete loss of Indian ways. Traditionalists showed variation in their reactions to the value of the conference and what it might accomplish, many seeing real benefits to be derived. Certain common themes ran through virtually all of the letters, as noted in the Coordinator's communications of March 16 and March 31. These reports and letters (Figs. 3 and 4) presented news and summarized developments. They also indicate what was happening generally with AICC. Among other things Indians were taking responsibility for the substantive phases of the work, even to substituting their own choice of name for the coordinator's "Charter Convention."

Meetings

Meetings were of four types:

1. Meetings with non-Indians called by Sol Tax himself or which he simply attended to publicize AICC among groups who could provide help in the way of information or services needed by Indians engaged in the project. The people Tax contacted in this way included anthropologists (in three separate meetings); other scholars; lawyers; representatives of government agencies concerned in one way or another with Indians such as the Indian Bureau, Department of Health, Education, and Welfare, Congressional and Senate committees on Indian affairs; representatives of religious denominations; and representatives of organizations devoted entirely or in part to Indian rights and welfare. Through these meetings AICC was in turn publicized widely as various organizations put their mailing lists at Tax's disposal or ordered bulk quantities of the materials to be sent out under their own cover letters indicating what special work members of the given or-

THE UNIVERSITY OF CHICAGO

AICC

SOL TAX, Coordinator
1126 EAST 95th STREET
CHICAGO 27 - ILLINOIS

REPORT OF THE COORDINATOR OF AICC

March 16, 1961

NANCY O. LURIE
Assistant Coordinator
3700 EAST DELHI ROAD
ANN ARBOR - MICHIGAN

My work as Coordinator is to keep everybody in touch with everybody else so that all of them will know what is going on, and be a help to one another. More and more people are becoming interested in AICC and want to catch up on the past history so they can take an active part now.

Many people have felt the need for a reassessment of Indian policy. Among these were the officers of the "Schwarzaupt Foundation" who for many years have been giving support to Indian education. In September, 1960 they asked if I could help to develop a statement about Indian policy that would be useful to all of the people who are trying to help Indians. I happened to know that the University of Chicago had some money especially for conferences to discuss important questions of public policy. I thought that this could become a great opportunity for Indians to develop their own statement of what Indian policy should be.

The first thing to do was to begin asking Indians about this idea. It so happened that the National Congress of American Indians had been asking how the resources of universities could be used by Indian tribes. They invited colleges and universities to send representatives to their November, 1960 convention in Denver. I was chosen to represent the University of Chicago at the Convention. This intertribal gathering of Indians gave me a chance to bring up the possibility of an "All-Indian Conference" to work out a statement of Indian policy. The people at the NCAI Convention thought it was a good idea. I reported to the University that we ought to go ahead. The University then appointed me coordinator to begin making the necessary arrangements for an Indian conference. The Schwarzaupt Foundation and the University of Chicago each put up \$10,000 for the preparations.

The only short word I could think of for the "statement of Indian policy" was the word "Charter." I asked the NCAI to make up a "sample Charter" so that Indian tribes could have something to start with, as an example of what they themselves might write up for the Convention. They set to work at once.

Meanwhile, I began to make arrangements with the University regarding space and other facilities necessary for a conference. On December 2nd I received the first "sample charter" written by the Committee of the NCAI. Immediately, I had it printed and mailed to Indians throughout the country. I also included copies of a map and some other documents. I thought Indian people might be interested in having them. Then, so that the Indians could draw upon help from many different sources, I sent the same material to universities, church groups, organizations interested in Indian affairs, and officials in Washington. As coordinator, this was as far as I could go. I waited for the people to discuss the "Charter" and tell me whatever ideas they wanted me to send around to others.

In order to get going on the actual conference, I invited a number of Indian people from different parts of the country to meet in Chicago in February. Each of them would understand some of the different kinds of problems Indians are facing. The Indians who came to Chicago had talked with others about the idea of an Indian "Charter Convention" and didn't like the words. They felt that the ideas were good but that the words "Charter" and "Convention" were misleading. They finally decided that a good title would be simply AMERICAN INDIAN CHICAGO CONFERENCE, "The Voice of The American Indian." This change was immediately made. The Indians who met in Chicago in February were not



ganizations might perform. Note should be made of the participation of non-Indians. Every effort was made to stress the role of non-Indians as resource people rather than as initiators of ideas for Indians.

2. Meetings of Indians called by Tax to help him in the work of the Conference. In addition to the meeting in February detailed in his report of March 16 (Fig. 3) a similar meeting was held in April, resulting in the second version of what was by then called "The Declaration of Indian Purpose." Members of the Indian Committees which took form in the course of these meetings also convened in Chicago several days before the Conference to make final plans and arrangements for its conduct. Tax also met several times with a committee of Chicago Indians serving as Conference hosts.

3. Regional meetings (Fig. 2) planned by the Indians and expedited by Tax who made arrangements for these meetings to be held at scholarly institutions located in each region, as explained in his report of March 16.

4. Community, tribal and inter-tribal meetings. As judged by post-cards and letters received, at least 250 such meetings were held all over the country as AICC materials arrived in Indian communities. Most of the inter-tribal meetings were held under the auspices of non-Indians somewhat on the pattern of the larger regional meetings. In other cases, existing inter-tribal organizations discussed AICC on their own initiative.

The Coordinators relied on all public media of communication in order to fulfill the objectives of AICC. The press, radio, and television were provided news releases, interviews and articles all through the period from December to the end of the Conference. More personal contacts were provided by Indians and others who travelled extensively for the purpose; for example, the American Friends Service Committee sponsored travel by the Lakes Region Indian Organizer; an Indian leader from the Southwest travelled in the far West to establish contacts; the Director of the

NCAI used all occasions to travel on behalf of AICC; anthropologists and other scholars spent weeks visiting their Indian friends.

Conference Preparations

Shortly before the conference the coordinating staff compared notes for an article which Tax had promised the *Chicago Sun-Times* (Fig. 5).

People had said one "couldn't get Indians together." Although the Indians were responding surprisingly well and confidence in the AICC seemed high, perhaps these were our own overoptimistic assessments. How many would actually come to Chicago for the Conference? Part of the answer was another question: where would they get the money? A few tribes could afford to send delegates; some religious organizations might subsidize the travel of Indians (the Episcopal Church in the end did most); but for the most part Indians would have to pay their own way. With the registration was announced a "distance subsidy" by which the expense of travel from far places would be compensated by lowered cost of room and board at the University. Thus Indians from the far West would pay only \$15.00 for the week, while those closer to Chicago would pay more on a sliding scale as the distance decreased.

During the final weeks of preparation, there were anxious moments. The Montana Inter-Tribal Policy Board, the Indian organization which had been among the first and most active in AICC had suddenly repudiated the entire endeavor and in terms that indicated total misunderstanding of everything that had gone on before. An influential Sioux group in South Dakota for no apparent reason consistently discouraged efforts to publicize and discuss AICC. In Oklahoma, a Southern state, racism seemed involved in worries over credentials and definitions of who should be considered an Indian. Finally, there was the continual danger that those traditionalists who had joined AICC activities (albeit warily) would withdraw entirely at the slightest hint of disparagement of their views by other Indians. But in the end "everybody" came, the leaders from Montana, South Dakota, Oklahoma, progressives and traditionalists, men, women and children.

The final registration showed that 467 Indians from 90 bands and tribes had officially taken part in the conference, paying the \$5.00 registration fee set by the steering committee and participating fully. Many Chicago Indians who did not all register but merely visited with friends who had come to

there to represent any particular group, but only to work out means of getting the development of the conference fully into Indian hands. It was not their job to take over control of the conference but only to see to it that every Indian in the U.S. has an equal chance to take part and be heard. They worked very hard and did a fine job. By setting up regional committees they made it possible for Indians at home to get together for discussions and work on their different local proposals. It is now up to me as Coordinator to see to it that the information which comes to me from the regions gets sent around for everybody to study.

A great deal of information has already been coming in to me from Indians all over the country and more will arrive after the regional meetings. I will try to get this information to all of you as fully as I can. However, if I were to have everything that came to me printed and sent out it would come to hundreds and hundreds of pages. I will try, therefore, to study it all carefully and report to you all the different points that people think ought to be known. Different Indian groups have different problems. For example, here are some of the different proposals that are coming in:

1. Some Indian people want to take all of the proposals of the NCAI sample program. They think it suits them the way it is.
2. Some Indian people want to keep the Indian Bureau just as it now is but want it to provide more services for Indians.
3. Some Indian people want just to live by their own treaties with little or no government concern in their affairs, except obligations defined by treaties.
4. Some Indian People who do not live on reservations feel that they have special problems and that the federal government has an obligation to help them.

In addition to suggestions that have come from different groups, sometimes there are differences of opinion within a single Indian Community. For example:

1. Some think that their present tribal setup is good; others think it is bad.
2. Some want their children to go to school on the reservation; others want their children to go to school off the reservation.
3. Some want the state to handle law and order on their reservation; others want Indian courts and police on their reservation.

4. Some feel that their most pressing needs are economic; others feel that their most pressing needs are spiritual.

Fortunately, AICC is not a contest where, among all the different Indian points of view, one has to be accepted over the rest. A program for all Indians could recognize and respect these different points of view and provide for all of them. For example, there are some very important things on which everybody seems to agree, and under which all of these different suggestions could be possible.

1. Everybody seems to want more local self-determination for Indians, whether they live on reservations or in non-reservation communities, whether they belong to "organized" tribes or "unorganized" tribes.
2. Everybody seems to want better educational opportunities for their children, whether they see this best accomplished by reservation or non-reservation schools.
3. Everybody seems to want the chance to make their local communities economically self supporting, whether by cattle raising, farming, manufacturing, lumbering, or some other ideas that they have.
4. Everybody seems to want the right to preserve their Indian communities and local traditions and to feel pride in being Indians, whether they have kept many of their old ways or only a few very meaningful ones.

Therefore I am optimistic that the drafting committees studying all the different points of view will come up with a statement on which there can be general agreement, and which will help everybody. This is possible because the general statement could take account of the different needs in different Indian communities. But this cannot be done fully unless all Indian groups prepare statements of their points of view and suggestions for general study. As Coordinator, I am at your service to receive your ideas and send them to others who are working on this conference.

If you cannot get to one of the regional meetings, you are welcome to send me directly your ideas and programs and I will see that they are circulated.

Good luck in your discussions!

Sol Tax
Coordinator
American Indian Chicago Conference

town, and many spouses of registrants, and dancers in the Pow-Wow, swelled the number of Indians at the University during the week to 800.

The Conference

The schedule for the conference had been planned in advance (Fig. 6). The format for the conduct of the conference though accepted at the outset by vote was necessarily adapted to the needs of the discussants, as they went along. The conference opened the afternoon of June 13 with a solemn calumet ceremony of welcome performed by a traditionalist Winnebago resident of Chicago who offered prayers and passed the pipe of peace to representatives of the nine regions and to Tax. This was followed by a short program of Indian dancing and an outdoor feast cooked by Indians. There was a feeling of general congeniality and pleasant expectation as small groups gathered to eat together and chat. The calumet ceremony was marred to some extent by the need for television crews to set their cameras in the way of some spectators. (As the days wore on the constant presence of reporters, newsreel and television cameras, etc. were taken for granted, and accepted because for the most part the press was interested in reporting the serious side of the Conference.)

The opening session set the tone of the entire Conference. Except for a short welcome by the Coordinator, who traced the development of AICC mainly to stress its Indian character, the entire meeting was in Indian hands. The chairman of the opening session (a Cherokee) is a Justice of the Supreme Court of Oklahoma, and the keynote speaker (a Santa Clara Pueblo), is professor of Anthropology at the University of Arizona. The keynote speech stressed the basic themes and goals of the Conference: Indian identity does not preclude living effectively amidst the complexities of modern life; Indian diversity does not preclude unity of action in securing Indian rights and working for a better life for all Indians. Edward Dozier addressed the group not as an anthropologist but as an Indian speaking to other Indians:

One of the important characteristics of us as American Indians is our enormous diversity. One has merely to look over this congregation to realize the fact, and when we examine the groups from which we come, the diversity is compounded. Yet, despite this variation, Indians have and continue to share a common situation. The very fact that there was an enthusiastic response to this general meeting of Indians is indicative of this commonality.

Our purpose at this conference is to compile a series of suggestions and recommendations. Here we must unite for it is the commonness of purpose that brings us together. While we differ as groups and

individuals our problems have a commonality. Pooling our efforts toward policies of greater benefits for ourselves is our goal.

If the opening remarks made clear the objectives of the conference, the conduct of business in the first session showed how those objectives were going to be reached throughout the whole course of the conference. In the brief time remaining before recessing until morning, virtually all of the major factions, issues, and personalities came into play. A pattern of real accomplish-

ment and agreement emerged suddenly, after a seemingly endless and aimless exchange of talk. This was to characterize the entire conference.

A series of rules had been prepared in advance by the steering committee and was submitted for a general vote. Immediately, individual items were questioned, the relative merits of voting on all or for each in turn were set forth by different people, the motives of the steering committee were questioned, the official delegates of federally recognized

FIG. 4. Letter from Dr. Tax dated March 31, 1961, explaining Coordinator's role in preparations for AICC

THE UNIVERSITY OF CHICAGO

AICC

SOL TAX, Coordinator
 NANCY O. LOHR, Assistant Coordinator
 ROBERT RIEZ, Associate Coordinator
 ALBERT WACHSBERG, Assistant to the Coordinators

March 31, 1961

To All American Indians:

The first time I addressed you was late in December, 1960, when I first invited you to the AICC, Chicago, June 13-20. Since then I have learned much.

I explained that the whole idea of AICC is to help all Indians of the whole nation to express their own views. I thought that you might be able to agree on a "charter"; but I soon learned that is not a good word to use. All I meant by that word was a written expression of the Indian point of view. I am glad that the name was changed--now we talk about the AICC as "The Voice of the American Indian." If a document comes out it will probably be called a "Declaration of Indian Purpose."

Some people originally got the idea that the AICC is some kind of new organization. As far as I am concerned, it is a single special opportunity--a national conference at the University of Chicago in June; maybe an important Declaration that will have great influence in how American Indians are treated--for once taking into account Indian needs in every community. That's all for the AICC; of course if you Indian people want to carry on in some way, we shall be glad to help if we can.

Some people also got the idea that I was under the influence of one group of Indians, or that I had some point of view of my own and wanted to make it seem as if it is coming from Indians. None of this is true. My own idea is only that Indians should be given a chance to express their ideas and to be heard. And I mean Indians of all groups.

For thirty years I have been learning from American Indians, and I am convinced that you have much to teach that is important for all of us. We have planned for you, spoken for you, done things "for you" and done many bad things to you. The least that we can do now is to listen to you, for once. Something really good could come out of that.

So AICC is set up (1) to give you a chance to speak up strongly and freely and (2) to get the general public, and organizations and officials, to listen to you.

As Coordinator it is my job to try to get all kinds of American Indians to the Chicago Conference. It belongs to all of you. So far it looks like we might succeed. Leaders of all factions and all points of view are discussing AICC: Eastern and Western Indians; reservation Indians, terminated Indians and city Indians; full bloods and mixed-bloods; more acculturated Indians and less acculturated Indians.

They all write to me, and I hope they will all see this through. Maybe if all groups stick it out they will not agree. The easy way is to get agreement among only one group that already agrees. But what good will a Declaration of Purpose do if it speaks with the weak voice of just one group of Indians? Things would continue to be done to Indians by getting a few of them to agree to something that the others don't want. This is not only wrong, but it also doesn't work; the people who do not agree won't go along with whatever it is, and that will end it.

So the big job is to get all groups to stick together now and speak with one voice.

The reason I think this can be done is that we have been studying the

The Voice of the American Indian

JUNE 13-20, 1961 AMERICAN INDIAN CHICAGO CONFERENCE

tribes exchanged pointed views with delegates of non-federally recognized tribes, changes were made, altered, and tabled, and after everyone had his say, the 15 recommendations were passed virtually as they had been presented by the steering committee. According to the minutes of the meeting, there were "no 'no' votes." Although almost all of the following rules were then disregarded at some point during the conference, they did provide the basis on which consensus could be reached:

1. The Chairmanship of the Conference will rotate each day. There will be six

general assemblies and for these, the Chairman will be selected from a panel of names.

2. The work of the Conference will be conducted in discussion committees. In order to provide maximum opportunity for everyone to participate, these discussion committees will consist of not more than fifty persons and therefore the total number of discussion committees will be determined by the number of participants in the Conference.

3. The discussion committees will meet for the first time on Wednesday morning, June 14th at 9:00 o'clock and will organize themselves by selecting a chairman and a recording secretary. Members of the Steer-

ing Committee will sit with these discussion committees and the Steering Committee member will act as temporary chairman until the discussion group is organized.

4. The discussion committees will report back to the general assembly each day at the evening session.

5. The general sessions on Wednesday, Thursday, and Friday will be limited to these reports and to general exchange of information and will not be parliamentary session. By this we mean that no formal vote or final action will be taken on subjects brought up for discussion.

6. The voting on the report of the Conference, or Declaration of Indian Purpose, will take place at the general session starting Monday morning, June 19th and on Tuesday, June 20th the document will be presented to the public.

7. It is the hope of the Steering Committee that as the Conference proceeds decisions can be reached in a spirit of unity and that formal balloting will not be necessary; however, the Steering Committee recommends that if a formal vote is necessary at the end of the Conference the votes of the official delegates from the federally-recognized tribes will constitute 60 percent of the total vote in the Conference; all other participants in the Conference will have one vote each and the total of these votes will constitute 40 percent.

8. A Committee on Credentials will be created, the members to be elected by the Conference. This Committee will determine and certify the official tribal delegates from federally recognized tribes, and the individual delegates.

9. A Rules Committee will be established, and its members will be elected by the Conference. This Committee will have the specific function of meeting with the official delegates of federally-recognized tribes and determining with them how they want to allocate the tribal votes among the tribes represented.

10. Roberts' Rules of Order will govern the procedures in the Conference.

11. A Drafting Committee will be established and will have the responsibility of coordinating and compiling the final report, or Declaration of Indian Purpose of the Conference. The membership of this Committee will be elected by the Conference.

12. The parliamentarians for the Conference will be the members of the Rules Committee.

13. Sergeants-at-arms will be elected by the Conference.

14. In order that there may be a governing body in existence until a final action is taken on a report, it is recommended that the Steering Committee will continue to function until a final report is adopted and made public.

15. A ballot will be distributed suggesting names for officers of the Conference and Committee memberships. These names were taken from the advance registration lists and selected for geographical representation. Nominations may be made from the floor and persons so nominated may be written in on the ballot.

In accordance with point 15 above, paper ballots had been prepared in advance to elect the members of the credentials, rules and drafting committees; as well as sergeants-at-arms, secretaries,

-2-

responses that you have been sending in. It looks as though all groups of Indians actually agree on some of the most important things such as

1. Indian ways are right ways for Indians;
2. The traditional rights of Indian nations have never been lost and should not be jeopardized;
3. Indians whose economic resources have been taken away need financial help but want to manage their own affairs;
4. Knowing that their Indian culture discourages avaricious behavior, some Indians will always want special protection against the loss of community rights and resources;
5. Indians want education, to take full advantage of the resources of the modern world, and they can get this without necessarily adopting the values of the white man;
6. Wrongs of the past must be undone wherever possible;
7. Present wrongs must be ended at once; and
8. Measures must immediately be taken to protect from now on all Indians' rights as they have never been protected before, and to help the Indian people achieve better health, education, and economic well-being.

If all factions among Indians really do agree on all 8 points, they can probably easily come to agreement on ways to carry out point 8, which refers to future programs.

As I read the correspondence that comes in, I think that Indian groups do agree on all 8 points; but the difference comes in this: The more "traditionalist" Indian leaders begin with the first one and want to be satisfied on each point in that order--1,2,3,4,5,6,7,8. They won't consider proposals under 8 unless these proposals are built on the first 7. On the other side, the Indian leaders who are working with Congress and the Administration begin with Number 8. They firmly believe in the other seven, but think it is better tactics not to talk about them with people who do not understand anyway; and they think that by concentrating on "program" they can get a better deal for Indians without endangering the first seven.

The "traditionalists" say that this is immoral, and un-Indian, and has never worked in the past, and it can only do harm to the whole Indian cause. The others reply that if Indians refuse to develop any program unless all old wrongs are first made right, Congress will continue to pursue a negative program and conditions will get worse instead of better.

Each side mistrusts the other side more and more, and there seems to be no chance of getting together. So what happens? The leaders who are willing to work on programs do so, but when there is disunity, strong enough programs cannot be gotten through and those programs that are tried are not successful on the local level. Each side continues to blame the other, mistrust and misunderstanding deepen, and things go from bad to worse.

There are two things I want to say about this. First, we in the Coordinator's office of AICC do not take sides. We are on the side of bringing the two sides together and we cannot do that if we ourselves favor one side or the other. We try to understand both sides and work to bring them together. Second, we think they can be brought together to make a single Declaration of Indian Purpose which will be the strongest possible Declaration because, if it is agreeable to all, it will necessarily have built into it every one of the 8 points in which all of the Indian people believe.

I hope that you will be thinking of this in all your discussions. The "Statement of Indian Purpose" will not be complete without YOUR voice.

Sincerely yours,

Sol Tax
Sol Tax
Coordinator

What The Indians Want

By Dr. Sol Tax

It may come as a surprise that there are about as many Indians in the United States today as there were when Columbus discovered America.

The aboriginal population of North America, including Canada and the northern border area of Mexico, is estimated to have been about 1,000,000. This population was greatly reduced by wars and epidemics, and by the end of the 19th Century it appeared that the Indians were a people doomed to extinction.

In the 20th Century, their numbers have been steadily and rapidly increasing. Between 1930 and 1960, the American Indian population—excluding Alaska—rose from 332,397 to 508,665, an increase of approximately 50 per cent, roughly the same as the increase in the nation as a whole despite the fact that Indian death rates are still proportionately higher compared to birth rates than is the case for the general population. It is striking that this increase continues even though all government programs consistently aim at decreasing the Indian population as such—in earlier years by military campaigns and for the last 75 years by promoting the disappearance of Indians into the general population.

Nor are 1961 Indians merely Indians by name or appearance. While we may pass many of Chicago's more than 4,000 Indians in the streets every day without even realizing that they are Indians, almost all of them live and guide their lives in terms of moral values more Indian than non-Indian. The "vanishing American" is here to stay.

THE SO-CALLED "INDIAN PROBLEM" has been with us since Colonial times. Although special bureaus, agencies and commissions in the federal and many state governments, as well as private organizations, have devoted vast amounts of money and endless expert planning to Indian affairs, the "problem" remains. One may well ask why.

To seek an answer to this question, Indians from tribes all over the nation will meet for a week beginning Tuesday at the University of Chicago. The American Indian Chicago Conference will climax six months of concerted effort by Indians themselves to make their own voices heard. We anthropologists who are co-ordinating the effort have set ourselves the task of listening and learning but not speaking.

In the last six months, I (for one) have learned more about the Indian view than in my 30 years of previous research. We do not know what proposals will be made at the conference nor the language in which they will be couched, but it is already possible to restate the "Indian problem" in terms of the underlying Indian point of view, to take into account not only the objective facts as known from historical and anthropological studies, but how these facts have been experienced and interpreted by Indians themselves.

FROM THE BEGINNING, Indians have cherished their heritages, their communities, their homelands, their moral values. More than anything else, they have wanted to retain their identity. Whenever they came to realize that Europeans had come to take away their land and their identity, they resisted. From the beginning, whenever programs have threatened their land or their identity, such programs have failed. Therefore, the first necessity in dealing with Indians in the United States honestly and intelligently is to stop trying to take away their land and stop trying to take away their identity as Indians. We must assume that every Indian tribe is here to stay.

Basing policy on any other assumption is like saying that the solution to the well-known school bus problem is for all Roman Catholics to become Protestants or vice versa.

This does not mean that we have to move back to Europe and give the continent back to the Indians. It only means that we must stop threatening the last remnants of Indian land holdings and strengthen the Indians' land base wherever we can.

Nor does this mean that Indians expect to live by hunting, fishing and handouts from the federal government or anybody else. It only means that we should return to Indians, for as long as needed—and this may be forever—some equivalent to the economic opportunity that was lost to them with the loss of their lands.

Nor does the requirement that their identity be respected mean that Indians want to turn back the clock and live in the manner of their ancestors at the time of European contact.

RESPECTING THE IDENTITY of Indians means recognizing the continuing existence and identity of Indian communities. Nobody is "just an Indian." He is, first of all, a Tuscarora, or an Apache, or a Menomoni, or a Klamath or a member of some other tribe with its particular history and traditions. Before the white man came, there were Hopis and Winnebagoes and many other tribes, but no "Indians." It is only the Europeans who saw them all as "Indians." Therefore, a threat to the existence of the community is a threat to the existence of every member of the community.

Some Indian people resent having been "given" citizenship by Congress in 1924, seeing in this act the possible destruction of their tribal integrity and individual rights. The implication, such as in the word "assimilation," that a tribe and its culture will eventually disappear, is a threat of death to every individual concerned. No wonder the long-standing assumption that Indians will welcome assimilation has paralyzed attempts at constructive programs for Indians.

If a change, no matter how beneficial, is defined as a change from being Indian to being white, it stands a good chance of not being adopted. If the opportunity to learn a trade suggests the beginning of a process of departure from being an Indian, it may well be rejected. Precisely because the rifle, the horse and the automobile were never associated with assimilationist interpretations, they were incorporated into Indian cultures. Frequent failures of "government programs" may be traced to justifying them as a means of assimilation.

Before money is appropriated, we have always wanted assurance that it will help Indians to stop being Indians in the expectation that then they will no longer need help. Some Indian leaders themselves have considered it expedient, and even necessary, to play along with this expectation in attempting to get help for Indians in desperate need. As a result, they may lose the confidence of members of their groups who feel that these leaders are selling their Indian heritage for a mess of government potage.

New and better understanding and some 300 years of historical perspective point out not only our past errors but how effective it would be to approach the Indian problem in a totally new light. It should be easy enough now to stop taking the land, since we have taken all but a tiny fraction of the entire continent from the Indians, and perhaps Indians should now let bygones be bygones. How can they let bygones be bygones when they have not yet gone by? The age-old effort continues to alienate Indians from their few

remaining acres under the same old pretexts that it is for their own good. Indeed, it even bothers us to let them have some few bits of their remaining lands free of local real estate taxes, thinking it too "generous" or "discriminatory."

Individual Indians never had the right to alienate tribal property, and there is probably no case in our history when a tribe responsibly and willingly parted with the territory it used and occupied as its own. The continent was therefore taken away by hook or by crook. We who think of land as real estate do not understand or appreciate the continuing and poignant personal loss felt by Indians who lose their lands. While my work and association with Indians made me aware of the sacred tie of the Indian to land, the last six months have revealed its unsuspected intensity and universality. A tribe without its land is as inconceivable as an Indian without his tribe.

Since the time of earliest contact, it has been our bland and naive assumption that Indians would not only part with their lands as so much real estate, but would jump at the slightest chance to shake off their curious customs and strange ways to become like Europeans. The psychological reason for this myth probably combined the hopes that Indians living like Europeans would need only a few acres per family and there would be more than enough land for all; and that European farmers would be spared the disconcerting example of Indian neighbors hunting and fishing while they work on routine farm tasks.

IN ORDER NOT to waste the taxpayer's money and the Indians' time in futile enterprises based on the expectation of eliminating the Indian problem by eliminating Indian communities, government agencies must reconcile themselves to the historic fact of Indian persistence and develop an appropriate administrative philosophy. All who are concerned with Indian welfare must be reconciled to the need to subsidize Indian communities and to help Indian individuals to make their ways as Indians. How much less subsidy than at present will ultimately be required we will know only after the Indian people receive opportunities to develop their resources in an unthreatening atmosphere of free choice.

The famous case of the Mohawks, who have made the high steel industry uniquely and peculiarly their own specialty while retaining and reaffirming their identity as Mohawks, illustrates that Indians can adapt effectively to the most modern conditions—given opportunity and free choice. The Mohawk case was not a government program—it developed fortuitously—but it offers a model and a philosophy in solving the Indian problem.

RECOGNIZING THE IDENTITY of Indian communities and their right to persist means more than only removing threats. It also means allowing the Indian communities to become again whole and fully functioning. When Indian communities were independent and sovereign, they were able to adapt readily to changing conditions and avail themselves intelligently and efficiently of new ideas, techniques and material objects. Throughout the long period of the fur trade, for example, Indians enriched their cultures without losing their sense of identity.

In our management of Indian affairs we have made two serious miscalculations. First, we, rather than the Indians, have decided what their goals should be. Second, we have tried to see to it that these goals are reached by our own

rather than by the Indians' methods. Important community decisions are made by outsiders and the work of carrying out the decisions is done by outsiders. Since a normal community derives its meaning in the very act of organizing to make decisions and to carry them out, all American Indian communities have been effectively crippled. One result of this has been that normal differences of opinion cannot be worked out within the community.

Since decisions are made outside the community, people with inclinations and skills for leadership can only compete for power rather than resolve issues and carry out responsibilities. Then, compounding the error, we blame tribes for their "factionalism" and for the "lack of true community leadership"; and we claim still more the need to make their decisions for them "because they can never get together."

IT IS SOMETIMES ARGUED that the crippling effects of governmental paternalism could have been avoided if the government had simply "stayed out of the Indian business." But this has not worked either. There are many Indian communities which are not recognized by the government which face problems as acute as those of the over-regulated communities. These "non-reservation" settlements, even when they no longer "own" their land, are identifiable as communities whose members are as attached to their territories as any other Indians, and with pressing problems comparable to the others. Their problems cannot be wished away by refusing to recognize them. These are communities without paternalistic control but also without the needed subsidization to begin to carry out choices they would like to make for their own benefit.

IF THE FOREGOING ANALYSIS is correct in regard to Indian communities generally, whether recognized or unrecognized by the federal government, and if it properly takes into account the Indian point of view, it appears that the responsibilities of government agencies are clear:

- 1 Indian communities should be subsidized without setting any time limits.
- 2 Subsidization must not interfere with effective Indian selection and execution of their own programs.
- 3 Subsidization on the same terms must include legitimate, traditional Indian communities that perhaps never have been subsidized.

It is said that Congress, at least, will not consider proposals so different from the established concepts of Indian administration, ineffective as these concepts have proved. I assume, however, that people are educable and that Congress is responsive to informed public opinion.

The Indians meeting in conference in Chicago this week are eager to bring their problems as they understand them before the American public and to tell us how they are prepared to work them out.

Will these Indians be satisfied to find solutions limited by the established channels of administrative precedent in the belief that this is their only possible recourse? Or, will they rather strike out in a direction which is unprecedented but necessary from their own point of view in the hope that the American public will, for the first time, hear and be impressed by "the voice of the American Indian" on the Indian problem? In either case, the choice will be a decision of the Indian people themselves and thus a necessary first step toward any good solution of the age-old "Indian problem."

Fig. 5. Article by Dr. Tax appearing in *Chicago Sun-Times*, June 11, 1961. Reprinted by permission of the *Chicago Sun-Times*

escorts, and chairmen for each day of the conference. Names of people chosen by the steering committee were placed in nomination on the ballot and space was left for further nominations from the floor. The steering committee had made its choices from the names of people who had registered for the conference in advance. When the matter of voting was taken up questions and discussions again ensued and it was noted that some of the people listed on the ballot had not yet arrived. It was agreed that volunteers willing to serve on given committees should be determined by a committee appointed by the chairman and that their names would be placed in nomination from the floor. Voting was postponed until morning. Meanwhile, the present chairman should serve until regular chairmen were elected. The entire group would reconvene the next morning before breaking up into small discussion groups.

Wednesday morning, nominations were further discussed, and after everyone was satisfied that no special interest group was trying to take over the credentials, rules and drafting committees, the paper ballots were discarded and the offices were all filled by election based on standing votes. After it was determined that only nine small discussion groups or committees would be required, the group was ready to get to work. A review of the above details indicates that of the rules to which the group bound itself at the outset, 1, 3, 15, and, in effect, 10 were either disregarded as unnecessary or adapted to the needs of the group as the needs occurred. This was typical of the entire conference. The steering committee took elaborate measures to maintain an "open" conference and give assurance that everyone's rights would be protected. Once the group was satisfied that these requirements could be met

without the elaborate measures, they were replaced by simple and direct methods in the conduct of business.

Perhaps the most dramatic example of this approach was in regard to rule 7. Arguments concerning voting and representation had begun at the meeting held in Chicago in February and continued until the April meeting, when rule 7 was finally agreed upon. The reason for concern is apparent if the discussion of factionalism is recalled. Official delegates of federally recognized tribes with sometimes vast assets at stake were fearful of their own discontented tribesmen on the reservations and of their off-reservation members. Non-reservation Indians in general, and the presence at the conference of non-federally recognized tribes, also evoked their anxiety.

During a large part of the meeting in June, the pros and cons of voting were debated even though the 60%-40% di-

Fig. 6. This pre-Conference schedule, with few changes, became the AICC program

REVISED PRELIMINARY SCHEDULE

	TUESDAY JUNE 13	WEDNESDAY JUNE 14	THURSDAY JUNE 15	FRIDAY JUNE 16	SATURDAY JUNE 17	SUNDAY JUNE 18	MONDAY JUNE 19	TUESDAY JUNE 20
9 am		GENERAL SESSION 1 REVIEW OF HISTORY OF INDIAN AFFAIRS	GENERAL SESSION 3 PROGRAMS FOR THE FUTURE	GENERAL SESSION 5 THE AMERICAN INDIAN CREED	GENERAL SESSION 7		GENERAL SESSION 8	GENERAL SESSION 9
10					READING OF PROPOSED "DECLARATION OF INDIAN PURPOSE"	INDIAN PRIESTS WILL CONDUCT MASS: St. Thomas - Roman Catholic St. Paul's - Episcopal	AMENDMENT AND DISCUSSION OF DRAFT OF "DECLARATION OF INDIAN PURPOSE"	PRESENTATION TO PUBLIC OFFICIALS OF "DECLARATION OF INDIAN PURPOSE"
11						SERVICES UNIVERSITY CHAPEL		
12								
1 pm	Conference begins after lunch							(Conference ends with lunch)
2								
3	ASSIGNMENT OF ROOMS AND CHOOSING COMMITTEES	1 2 3 4 5 6 7 8 9 10 COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE	1 2 3 4 5 6 7 8 9 10 COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE	1 2 3 4 5 6 7 8 9 10 COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE COMMITTEE	BASEBALL GAME CHICAGO WHITE SOX vs MINNESOTA TWINS 1:30 P.M. FREE TO A.I.C.C. REGISTRANTS			
4		(INSTITUTE ON INDIAN AFFAIRS 2 P.M. TO 5 P.M. NON-INDIANS AND THOSE NOT BUSY IN COMMITTEE WORK)				INDIAN POW-WOW AND DANCE CONTEST 2 P.M.		
5								
6	OPENING "CALUMET" CEREMONY							
7	WELCOME FEAST	GENERAL SESSION 2	GENERAL SESSION 4	GENERAL SESSION 6	OPEN TIME ENTERTAINMENT PROGRAM?		FINAL VOTE ON "DECLARATION OF INDIAN PURPOSE"?	
8	KEYNOTE SESSION	COMMITTEE REPORTS	COMMITTEE REPORTS	COMMITTEE REPORTS	RELIGIOUS OBSERVANCES? INDIAN SINGING?	PICNIC SUPPER		
9								
10								

vision had been voted on as a rule of the conference, to be used if needed. The various factions voiced their views, feelings, and suspicions of each other. At one point, the official delegates of federally recognized tribes held a special session for which a general meeting was temporarily recessed. They quickly came to agreement that they would count their 60% as a bloc vote bound by majority (51%) decision within the bloc. This triumph of fast agreement was met with dismayed arguments by those in the 40% group who saw that the others had given themselves power of veto. Later, non-reservation Indians of all types (some of whom were federally recognized and had official delegations, but were fearful of the more powerful reservation groups) requested that they be allowed to form a tenth, temporary, discussion group to work out their special problems. They were allowed to meet as a group long enough to present their findings in accordance with rules 4 and 5, even though it had been agreed earlier that each discussion group would take up all issues to be included in the *Declaration of Purpose* so that everyone would have a chance to express his views on all topics, and hear other views.

However, as people talked among themselves in the small discussion groups and expressed their opinions in the general sessions, mutual understanding began to develop and fears and suspicions began to abate. This was not accomplished easily. There were only a few days to overcome hostilities built up over some 150 years of past history, so it is not surprising that bitter and impassioned oratory marked the discussions. What is truly remarkable is that by the time all sides were heard, the complicated machinery for voting was never used and by general agreement the following statements were included in the *Declaration*:

The United States has long followed a policy of turning its back on Indians who are not physically in residence on a federally established reservation. Such a policy fosters the illusion that the Indian population is smaller than it actually is, but a more serious consequence is that it also perpetuates the illusion that the problem of assisting Indians is less than it is. The Bureau of Indian Affairs has even been known to take credit for those non-assisted Indians as examples of its accomplishments in promoting "assimilation." With the exception of some few individuals, these Indians, as they and their white neighbors know only too well, have not been assimilated; they have only been forgotten.

The several categories of Indians referred to above were cited: Indians who had lost all or part of their lands because the lands had been inadequately secured to them by the government; Indians living away from their lands because of economic necessity; Indians

who never had reservations; and Indians who are not under the federal jurisdiction. The section continues:

We say emphatically that problems of health, education, economic distress, and social non-acceptance rest as heavily on all the Indians in these categories as they do on the reservation Indians, and possibly more heavily.

Therefore, in all the Recommendations herein, it is to be understood that even where non-reservation and off-reservation Indians are not specified, it is our purpose to insist that their needs be taken into account.

It may be argued that the majority of federally recognized reservation Indians merely acted out of enlightened self-interest as they at last came to understand the old principle of "divide and rule" in its application by the government to different kinds of Indians. However, this would be an unfair appraisal of the sense of the conference. Results such as those quoted above came about through Indians getting to know one another as people, each of whose problems were but parts of a larger problem. Such understanding occurred through their opportunity to confront one another, to state their own cases and to learn how they had misunderstood other cases. This, of course, was a hoped-for result of the conference. More important, however, than simply reaching agreement was the way in which agreement was reached. Here was a large inter-tribal group, diverse in backgrounds and painfully conscious of this diversity, coming together because they shared common problems. As the meetings progressed, it became evident that Indians generally share a common technique for dealing with problems. Because of linguistic diversity, English was the language of the conference, and according to Rule 10 business was to be conducted according to a well-known format, Robert's Rules of Order.

However, it was soon evident that while Robert's Rules of Order were used when convenient this large and diverse gathering was conducting business according to another procedure, that at a general tribal council meeting. Although the Indians became exasperated with what some people said and how often and extensively they said it, the group as a whole found one another's approach to problems reassuringly familiar and predictable. There were parliamentarians in the group, some of them on the rules committee, who openly expressed their annoyance at the disregard of formal procedure, but they increasingly accepted their role as score-keepers rather than umpires in clearing up lines of discussion when the group occasionally

talked itself into confusion as to the specific issue to be voted on. On one occasion at a plenary session it was agreed to place a time limit on each speaker's presentation, but this was never rigidly enforced.

At only one time, toward the very end of the conference, was the patience of the majority of those present tried to the point of taking a drastic, but in fact rather meaningless, stand against the view of one faction. The group had earlier listened patiently and considerately to statements that American citizenship is not a good thing for Indians and that Indians should have their former status as sovereign tribes restored as part of the effort to obtain the government's respect for Indian treaties. Treaties are, after all, entered into between nations, not between a nation and a group of its own citizens. However, the final extreme, though eloquent, statement of this position made it clear that the group was dealing with more than factionalism based on mutual misunderstanding or factionalism based on the range of willingness to trust and cooperate with the government. Here was factionalism based on absolutely irreconcilable basic assumptions. Either you worked out programs recognizing a fundamental relationship of allegiance to the United States so as to enjoy certain rights under the government, including promises made in treaties, or, in effect, you seceded from the United States.

The politically shrewd Indians were horrified at such "seditious" sentiments, fearing that all Indians might somehow be penalized for views expressed by a few people. Others considered the stand harmful mainly because they felt it to be unrealistic and an obstruction in the path of possible achievement. By majority vote, it was decided to expunge the speech "from the conference record," but it was never clarified whether this vote meant that no mention of this opinion was to be made in the final *Declaration*, the taped record, or the written minutes. At any rate, it did not appear in the *Declaration*, although the rights of those who stood at the other extreme were at least respected in passing: those who choose to become completely assimilated into the dominant society and culture should not be hindered from doing so as individuals.

Indians who hoped that the conference would be devoted to establishing a new nation or series of nations standing only in a treaty relationship to the United States will naturally consider the AICC a total failure. It is true that only a small minority of Indians scattered among a number of tribes express

this view openly, but the extremely hostile reaction of the majority of those present may disturb less extreme or outspoken traditionalists and perhaps many non-traditionalists who simply keep their own counsel on the matter of citizenship. For good reasons, many Indians are hostile toward the government. There has been a long history of unfulfilled obligations and broken treaty promises. Although the final document gives expression to grievances against the government in emphatic terms, it nevertheless stresses citizenship and patriotism. The basic contention of the *Declaration* is that if the government and the people of the United States are finally enabled to understand and respect Indians, it will be possible to correct old wrongs and begin to work together in a mutually satisfactory fashion. This theme occurred frequently in the discussions when Indians mentioned the existence of many minority groups in the United States, especially the Jews, who are loyal citizens able to participate in the general culture and yet maintain distinctive customs and traditions.

Consideration of the attitudes of Indians toward the government and the American people raises the question of Indian feelings toward Whites in general. Certainly Indians consider themselves different from Whites, some defining the difference in coldly anthropological terms of traditions and values, others believing in a mystical racial difference. (Many Indians who hold this second view are of mixed ancestry but

believe that their Indian "blood" or "spirit" dominates although genetically they may be only a quarter Indian or less.) One of the observable differences is that Indians are usually withdrawn in the presence of strange Whites and take a long time to decide whether a White is worth their trust and close friendship. Respect for this trait led to a curious incident at the conference which revealed a range of feelings underlying similar behavior toward Whites.

About 145 non-Indians attended the conference, including anthropologists, lawyers, representatives of religious denominations and other organizations concerned with Indians. Their role as observers was strictly adhered to and they volunteered no comments, sitting quietly in the general sessions and attending the small discussion sessions. Most, but not all, counted a few Indians at the conference among their close personal friends. In addition to attending the Indian meetings, they formed their own discussion group (attended by a few Indians), designated the Indian Institute, where they could exchange ideas and listen to talks by anthropologists and others in order to expand their understanding of Indians. It was pointed out among themselves that while their presence was virtually unnoticed in the large plenary sessions, they were quite obvious in the small discussion sections. Since Indians who might be timid about speaking up in the large meetings nevertheless used the smaller groups to make their views

known for general consideration, it was suggested that strange Whites might cause self-consciousness in the frank expression of opinions. The non-Indians thus decided to exclude themselves from the small discussion groups. They did so reluctantly because these gatherings were usually more calm and easier to follow than the plenary sessions and furthermore evoked more specific factual information which was reviewed in the course of developing general recommendations. However, the non-Indians felt they had made a wise decision and it was conveyed to the conference at large.

The news was greeted by some initial expressions of appreciation for this effort to respect Indians' feelings, but one or two of the speakers included some rather strong and generally anti-White sentiments which sparked a heated discussion. Some Indians observed that the Whites who were present were sincerely interested in Indians as shown by their willingness to pay a high registration fee, and they had come to find out what is troubling Indians. Such people, it was felt, would not be offended if Indians criticized Whites. For the benefit of those who harbor unqualified grudges against Whites, pointed mention was made of the university students and others who were working day and night on a completely voluntary basis to type and mimeograph materials Indians needed to conduct the business of the conference. Several Indians expressed the hope that the conference would promote wide understanding of their problems and believed that if White people could freely observe everything, they would be better informed and able to educate other White people upon returning to their homes. Other Indians noted that the Whites at the small discussion meetings had been so polite and unobtrusive that it was possible to forget their presence and hold completely frank discussions. Finally, it was pointed out that many of the White people present were experts in one field or another such as law or government, and the Indians considered it part of the plan of the conference to have such people readily available to answer specific questions raised by Indians in their discussions. It was understood the experts would not interpose with gratuitous advice, but would provide facts and information as required by Indians.

When put to a vote, it was agreed that White people could attend the small discussions, but it was also clear what sort of behavior was expected of them. Had the Indians operated like most Whites and put the matter to a vote almost immediately, the result would have probably been the same. How-

Students of the Workshop on Indian Affairs sponsored by American Indian Development, Inc., leaving Chicago for the Workshop at Boulder, Colorado, June 20, 1961



ever, those Whites who are inclined to assert themselves might have taken it as an indication that they could also participate in discussions, while those inclined to be overly sensitive about giving offense might have been reluctant to speak even when called upon for information.

White Americans imbued with the idea of "getting down to business" are inclined to be impatient with Indians who will spend long hours just listening to all sides and who will allow discussions to veer off into seemingly unrelated details. Nevertheless, when Indians do "get down to business" it can be handled with speed and ease so that the entire process is in truth no more time consuming than White "efficiency." This was reflected in the fact that paper ballots were never used throughout the conference and voting took a minimum of time. Quantitative precision and even secrecy were unnecessary. People simply rose to be counted or raised their hands, and in many cases a voice vote sufficed to formalize for the record a decision already apparent to the group.

When the *Declaration of Indian Purpose* was read toward the close of the conference it was largely a matter of reviewing everything which had been worked out a bit at a time and agreeing to the whole as assembled and worded by the drafting committee. The document in its final form showed the Indians' overwhelming inclination to seek solutions to their problems within the existing administrative framework, leaving radical alterations of bureaucratic organization to the future as further study and circumstances might dictate. The Indians recognized that it was not necessarily the structure of the Indian Bureau that needed overhauling, but the attitude of government personnel. They also recognized the need to bring their case before the public at large in the hope that an informed America would understand and support their contentions.

The Conference Result

The *Declaration* opens with a Statement of Purpose followed by a Creed, both of which make clear the general principle that Indians have a right to retain their spiritual and cultural values. This, they affirm, is in keeping with democratic concepts of government and they state that the problems for which they seek solutions stem largely from disregard of this right and even from efforts to deny them this right.

The main body of the document is devoted to a series of general sections in which specific problems are detailed and recommendations for their solution set forth. Running through all these discussions is insistence on Indian

participation in planning their own programs and carrying them out. Technical assistance should be rendered by people who are not only excellently qualified in their special fields, but also able to understand Indians and make themselves understandable to Indians. Finally, the broad principle of Indian participation requires respect for the fact that Indians will require different programs from place to place as these are suited to their greatly varying situations.

The general sections of the document are: Legislative and Regulatory Proposals; Resource and Economic Development; Health; Welfare; Housing; Education; Law and Jurisdiction; Indians to be Served; and Indians and Non-Federal Agencies. The development of the first category is noted in figure 1. The following topics, through Law and Jurisdiction, were handled fairly easily. There had been agreement from the start that these problems were widely shared and that satisfactory solutions could be found if programs were adapted to community needs as these needs are understood by Indians. As noted earlier, the section, Indians to be Served, is a striking example of the way a highly controversial issue was effectively handled by the group. The last section should be of special interest to any reader who makes regular contributions to the furthering of good works in behalf of Indians. The document makes some telling points:

It is hoped that in the future there will be more joint undertakings by government and private agencies. Trusteeship responsibility need not be abandoned in order to encourage increased participation by groups and individuals outside of the federal household. And such increased participation can be of great benefit in promoting research in basic problems, in conducting pilot projects to test out practices and methods, and in providing learning situations for the Indian people. . . .

Such agencies [local governments, churches, missionary groups, private organizations, universities and scientific organizations] might well study these suggested programs [set forth in the document as a whole] and discover ways in which their resources and abilities can be utilized in joint efforts with the tribes, the federal government, and other interested groups. The Indian people generally will welcome technical assistance, but to be most useful the offered services *should be planned with Indians* and made part of a total program.

Anthropologists may well ponder their relationships with Indians upon reading the above statements. (It should be noted that for many Indians AICC was something of an education in the full nature of anthropology. The criticism has been made, sometimes bitterly, that while anthropologists are usually congenial persons, the discipline uses

Indians but is of little use to Indians in return.)

The carefully wrought account of problems with the recommendations for their solution must forever put to rest the notion that Indians are incapable of understanding their problems and of recognizing their own best interests. But if the Indians put their intellects into the earlier parts of the document, they put their hearts into the Concluding Statement. Perhaps, as time goes by, this will stand as the most important result of the entire endeavor. Here they seek to change people's minds, not just their official behavior, in regard to Indian affairs. They presented a convincing case.

To complete our Declaration, we point out that in the beginning the people of the New World, called Indians by accident of geography, were possessed of a continent and a way of life. In the course of many lifetimes, our people had adjusted to every climate and condition from the Arctic to the torrid zones. In their livelihood and family relationships, their ceremonial observances, they reflected the diversity of the physical world they occupied.

The conditions in which Indians live today reflect a world in which every basic aspect of life has been transformed. Even the physical world is no longer the controlling factor in determining where and under what conditions men may live. In region after region, Indian groups found their means of existence either totally destroyed or materially modified. Newly introduced diseases swept away or reduced regional populations. These changes were followed by major shifts in the internal life of tribe and family.

The time came when the Indian people were no longer the masters of their situation. Their life ways survived subject to the will of a dominant sovereign power. This is said, not in a spirit of complaint; we understand that in the lives of all nations of people, there are times of plenty and times of famine. But we do speak out in a plea for understanding.

When we go before the American people, as we do in this Declaration, and ask for material assistance in developing our resources and developing our opportunities, we pose a moral problem which cannot be left unanswered. For the problem we raise affects the standing which our nation sustains before world opinion.

Our situation cannot be relieved by appropriated funds alone, though it is equally obvious that without capital investment and funded services, solutions will be delayed. Nor will the passage of time lessen the complexities which beset a people moving toward new meaning and purpose.

The answers we seek are not commodities to be purchased, neither are they evolved automatically through the passing of time.

The effort to place social adjustment on a money-time interval scale which has characterized Indian administration, has resulted in unwanted pressure and frustration.

When Indians speak of the continent they yielded, they are not referring only to the loss of some millions of acres in real estate. They have in mind that the land

supported a universe of things they knew, valued, and loved.

With that continent gone, except for the few poor parcels they still retain, the basis of life is precariously held, but they mean to hold the scraps and parcels as earnestly as any small nation or ethnic group was ever determined to hold to identity and survival.

What we ask of America is not charity, not paternalism, even when benevolent. We ask only that the nature of our situation be recognized and made the basis of policy and action.

In short, the Indians ask for assistance, technical and financial, for the time needed, however long that may be, to regain in the America of the space age some measure of the adjustment they enjoyed as the original possessors of their native land.

Eight supplementary appendices appear at the end of the document. The first, a history of Indian administration since the publication of the Meriam Survey, had formed part of the document proper in its earlier versions, but was expanded as an appendix and worded more strongly. Five of the appendices, in the form of general statements or resolutions, reported critical conditions among specific groups of Indians whose problems merit the sympathy and concern of all Indians. Of special note is the resolution regarding the Kinzua Dam which is to be built in violation of a treaty signed in 1794 and which will flood a large section of the Seneca Reservation in New York State. Included with the resolution is a telegram prepared by the conference and sent to President John F. Kennedy protesting the building of the dam. The seventh appendix is an interesting inclusion reflecting the ever widening horizons of the many small bands and tribes of Indians in the United States. It is the text of a letter sent by the director of the Inter-American Indian Institute in Mexico City calling for closer relations and sharing of ideals of Indians of all the Americas. The final appendix lists by name, address and tribe all the participants in the Chicago Conference.

Evaluation of AICC

A review of the *Declaration of Indian Purpose* clearly indicates that the conference of June 13-20, 1961 was successful in carrying out its objectives, justifying the Indians' efforts and Tax's faith in the methods of Action Anthropology. The question may fairly be raised, however, whether 460 Indians representing 90 tribes and bands can really speak as "The Voice of the American Indian." It cannot be denied that the entire effort will be considered futile by those Indians who for two quite different reasons see their only hope in abolishing the Indian Bureau. Indians who see the course of assimilation as inevitable for all Indians, not just good

for themselves, will reject the document, and it will also be rejected by Indians who want their relationships to the United States defined solely by treaty.

Even assuming, although the assumption may be unwarranted, that those who hold these extreme views are a small minority, there are still some 600,000 Indians in the United States comprising about 200 separate tribes and bands which the conference endeavored to represent. All records have not yet been completely assembled, but a careful reading of the list of participants indicates that they came from all regions of the United States, with official delegates from large groups such as the Navajo and the various Sioux reservations expressing the views of great numbers of people. Also encouraging is the fact that many small, scattered tribes and bands are noted in the list. The conference drew not only federally recognized reservation residents, but terminated, relocated, non-reservation, and non-federally recognized Indians. Observations at the conference revealed both men and women taking an active part in the proceedings as well as a range of the very young to the very old. Among the young people were a group of some thirty students en route to attend a summer workshop sponsored by American Indian Development, Inc., held annually and devoted to developing leadership among Indians. This Workshop on Indian Affairs, also a project in which the anthropology department of the University of Chicago takes an active interest, convened at Boulder, Colorado, immediately after the conference. There the students reviewed and discussed their recent experiences and observations. Several former workshop students chaired the small discussion sections at the AICC, while other young Indians who had become increasingly involved in the affairs of their local groups in some cases came as official delegates of their tribes.

That elderly people should be much in evidence at the conference was to be expected, given the respect many Indian groups accord the aged, but their enthusiasm as expressed in public statements and conversations with the coordinators was particularly heartening. These people had lived under the administrations of a long succession of Indian Commissioners and had experienced alternating hope and despair as one policy and program followed another. They were also well acquainted with the fact that many sincere Whites had often endeavored to help Indians, and yet could do little to alleviate generally bad conditions. Yet these elderly people, realistically recognizing that it would take a long time and much effort

to attain the objectives of the *Declaration*, gave thoughtful and deliberate voice to their feelings of optimism about the work of AICC.

The general feeling of goodwill and hopefulness was expressed in many ways at the Conference. The pace of work was maintained through an exhausting workday schedule of 9:00 A.M. to 10:00 P.M. The Indians joked among themselves about "Indian time," but if meetings never started exactly on time, they often ran beyond the appointed hour for closing. During meals when the group gathered in the dormitory dining hall, table companions could be observed in earnest conversation on various issues. Every evening a majority of the participants gathered to dance together and talk informally. The tribal cross section was a source of amazement even to Indians who regularly attend the large western Pow-Wows where many Indians gather from great distances. Many Indians met members of tribes they had not even heard of before, and names and addresses were exchanged between these new found friends. At the conference Pow-Wow, held Saturday, June 17, the Chicago Indians, in behalf of all, presented Tax with an enormous peace pipe faithfully copied from the design used on all AICC literature. There were many such spontaneous expressions of pleasure at the progress of the Conference.

For example, during a lull in work at one of the plenary sessions, a short play was performed, written and acted by Indians. It was a clever and hilarious satire on bureaucracy and bureaucrats entitled "Reverse History," in which the Indians were the dominant group trying to figure out the strange motives and customs of the White people in an effort to help them. On a serious note, at the close of the conference, Tax was given a feathered headdress in a solemn ceremony expressing the Indians' esteem for the coordinator and appreciation of his work in bringing AICC into being.

However, a complete appraisal of the success of AICC requires the consideration of activities since work began, over six months before the June meeting. A continuing difficulty was spreading the news of AICC quickly to all Indians and keeping the channels of communication open. A mailing list was built up from a variety of sources such as Indian Bureau records of tribal council officers, NCAI and other organizations' files, and repeated pleas for more names in the various materials mailed out. Eventually, the list totaled 5,500 names of which about $\frac{3}{4}$ were those of Indians. It was impossible to keep Indian and white names separate as they were often submitted without identification. How-

ever, in the interests of economy, when it was known that a large mailing of the first materials was going entirely to non-Indians, these names were not put on a permanent list until postcards provided for the purpose were returned expressing an individual's desire to continue receiving literature. Many organizations ordered bulk quantities of the first materials to send out under their own cover letters indicating how their members might be particularly able to help in the work of AICC. Other groups put their membership lists at Tax's disposal. The list shown with the regional map (Fig. 2) continued to grow in about the same proportions.

A startling fact emerged at the end of the conference which suggests that at least twice as many Indians were contacted with each mailing than the numbers in the address file would indicate. When plans were made to mail out the *Declaration* in its printed form, it was discovered that half of the people at the conference had not even been on the mailing list but had been kept informed of developments through someone else. Then, it must be recalled that many letters expressing views for inclusion in the Progress Reports were sent by people who were unable to get to the conference in person. Many letters were received conveying the good wishes of individuals and groups of Indians who just wanted to let the coordinator know of their interest and that only financial distress prevented their attending the conference. Other Indians were unable to get away from their jobs long enough to attend the Chicago meeting, but did manage to avail themselves of the opportunity to attend the regional meetings. In addition to the regional meetings there were innumerable community, tribal, and inter-tribal discussions of AICC.

Doubtless, some Indians have never heard of AICC because for one reason or another we never got their names and they missed notice of it through newspapers and other sources. Some Indians, interested in tribal affairs, cannot read or write and there is no way of judging whether efforts were made to inform them of AICC. It was noted with real regret that only one elderly Indian unable to speak English attended the conference and took part in the discussions with the help of interpreters. In the Western U. S., there are still many older people who speak little or no English and it can only be hoped that they were somehow apprised of AICC and conveyed their views through other people. Finally, there are probably many Indians who received AICC literature and dismissed it from their minds as another scheme to help Indians bound to fail like the rest. Considering

the state of Indian discouragement, the wonder is that so many Indians responded enthusiastically to AICC!

Thus, despite all of the qualifications noted, there is no question that AICC was brought to the attention of many people in virtually all of the separate Indian groups in the country. Letters to the coordinator and results of regional meetings and smaller gatherings indicate that large numbers of Indians of all ages, both sexes, and many tribes parallel to the distribution at the conference in June took an active part in the development of AICC. Their views underlay the second version of the document. Thus, the *Declaration* represents far more than the voice of the 460 Indians present in Chicago. Furthermore in writing the final *Declaration* the drafting committee reviewed the many opinions submitted by mail since the publication of the second version.

We may now begin to consider the prospects for effecting changes in Indian administration as a consequence of the *Declaration of Indian Purpose*. As already noted, AICC came into being when it did because of the promising political situation. Indian hopes were raised tremendously with President John F. Kennedy's appointment of Stewart Udall as Secretary of the Interior because of Udall's known sympathy for Indians. Udall's first action in regard to Indian affairs seemed to justify their hopes. Before appointing a permanent Indian Commissioner, he chose a 4-man Task Force to travel about the country to study Indian needs at first hand by talking with Indians directly. This type of approach had been tried before, sometimes with the result that investigative bodies came back with facts but also with their old predilection for considering facts in terms of The Indian Problem. However, Udall's move was considered a good one, particularly since appointment of an Indian Commissioner would be made on the basis of the information collected from Indians. The Force was headed by W. W. Keeler, a prominent Oklahoma Cherokee Indian, and included William Zimmerman, Jr., once Acting Commissioner of Indian Affairs and identified with the philosophies of the Collier administration, Philleo Nash, an anthropologist active in the field of applied anthropology, and James Officer, an Indian Bureau employe with broad knowledge of Indian affairs.

Udall knew of the existence of AICC even before he was appointed Secretary of the Interior and was most favorably impressed with the idea. Thus, the Task Force and AICC operated in a cordial

relationship of mutual interest in Indians without impinging on one another. Zimmerman attended the Indian steering committee meeting held in April in Chicago as Udall's personal observer rather than as a member of the Task Force, and both Zimmerman and Keeler attended the June conference. By that time the tour of duty of the Task Force had ended, but Keeler addressed the group briefly—at their invitation—and expressed the great interest of the Task Force, Secretary Udall, and the government in general in AICC. However, the coordinating staff, at least, experienced some misgivings that perhaps the new administration as a whole was not going to pay much attention to Indians, despite Kennedy's campaign statements. All efforts failed to have Kennedy or his personal representative, someone not identified with the Interior Department or the Indian Bureau, come to Chicago and receive the final document in behalf of the American people. At the close of the conference, disappointment was somewhat softened when a telegram was received from Udall stating that arrangements were being made for a White House ceremony when the document would be presented. At this writing, the Indians have chosen their delegation and are awaiting word of the date of the ceremony.

One of the most encouraging signs that the Indians will get a fair hearing of their views is that on August 1, 1961, President Kennedy appointed Philleo Nash as Commissioner of Indian Affairs; the appointment is now pending confirmation by the Senate. As an anthropologist, Nash has special understanding and interest in the *Declaration of Indian Purpose*. On the other hand, since the conference evoked such good feelings and is firmly established in Indian minds with anthropology and anthropologists, Nash may be able to take office in a better climate of Indian opinion than any commissioner since the founding of the Indian Bureau!

The Commissioner of Indian affairs has a difficult and twofold task in accomplishing his ends. He must work with Indians who suspect he wants them to do things they don't want to do, and he must work with Congressmen who suspect he wants *them* to do things they don't want to do. Whatever its faults and shortcomings, however, the Indian Bureau is at the mercy of Congress as Collier and others have learned to their sorrow in efforts to obtain appropriations and proper enabling legislation for their programs. Often the Indian Bureau has stood on the side of the Indians only to find its recommenda-

tions ignored and protests disregarded as the legislative arm of government worked out "economy measures" to transfer or curtail Bureau services. Following Collier as Acting Commissioner of Indian Affairs, Zimmerman had worked in vain against the growing sentiment for termination. The Indian Bureau is often made the scapegoat for the failure of programs that were simply not properly financed and for programs it did not approve but had the responsibility of enforcing. The Congress has often represented itself as "the Indians' friend" in setting up legislation to at last "free the Indians" from the wicked Indian Bureau. Thus, the Indians' *Declaration* can at least provide future Indian Commissioners with a new weapon in combatting misrepresentations of Indian wishes. Udall's wisdom in undertaking an immediate and systematic study of Indian affairs by means of the Task Force will provide the objective "expert" information Congress will also insist upon. It is not surprising that the findings of the Task Force should parallel those of AICC if for no other reason than that many groups sent copies to AICC of statements they had prepared for the Task Force so that other Indians as well as the government would know their problems, needs, and recommendations.

Nevertheless, as both the Indians and Indian Bureau ruefully acknowledge, if Congress sets its mind on a given course such as termination, pure reason and Indian desires can be cast aside. Therefore, in their *Declaration*, the Indians seek to state their case not only to the government but to the American public in the hope of winning their support. Congressmen are elected to their offices, and are exceedingly sensitive to the feelings of their constituents. The Indians have cause for hope insofar as the American people are generally sympathetic to Indians. Most Whites are quite aware that the Indians were forced to relinquish their lands for a small fraction of their monetary value. The reason that there was no great public outcry in the last decade against legislation that threatened to consume the Indians' remaining lands is that on paper, anyway, it appears that Indians are being offered a fair price. More important is the fact that the dominant segment of the American population simply does not realize that their way of life is not sought eagerly by everyone else. Efforts to de-

stroy Indian distinctiveness and "make them just like White people" are viewed as laudable and as the least the Whites can do in making some decent restitution to Indians for having taken their country. These attitudes are going to be extremely difficult to change because they are held piously.

However, it appears that Indians are already beginning to accomplish their objective. Their case is finally being presented fully and fairly to the American public. It should be pointed out that in news media, Indians are traditionally "good copy." Until very recently, newspaper stories about Indians were either dramatically touching accounts of critical poverty, or lent themselves to eye-catching captions. In the second category, an Indian tribe never puts its case before the President but before "The Great White Father." Where other Americans "protested" or "took legal action," "Red men" always "went on the war-path." Stories following such headlines often stated the facts fully, but the reader was usually left with an impression of quaint aborigines trying to get "wampum" (never money) out of Uncle Sam. Indians, unlike other minority groups in the United States, seldom protested such stereotyping largely because it was never meant to be derogatory, although it has been damaging in clouding the public's understanding of Indians.

It was, therefore, with real satisfaction that the coordinators noted that with few exceptions news accounts of AICC from its very inception respected and stressed the seriousness of the undertaking. At first this may have been due to the austere name of the University of Chicago associated with the endeavor, but, as time went on, it was clearly because the newsmen were getting to know the Indians better. Indians became newsworthy not because they were quaint but because they turned out to be not quaint at all, but rational human beings with complex and unusual problems. Accounts in newspapers and television broadcasts showed pictures of Pow-Wow dancers, but even the Pow-Wows were gradually understood as something more than money making productions. Virtually every newspaper, from the University of Chicago student paper, the *Maroon*, to the nationally read *New York Times*, carried accurate and sympathetic accounts. Several points were stressed: AICC is unpre-

cedented in size, scope, and plan to find out what Indians want; Indians want things most Americans never knew they desired; and their desires are both realistic and understandable. Nor have references to AICC ceased now that the conference is over. Reports of current developments in Indian affairs, independent of AICC, have been referred back to statements in the *Declaration of Indian Purpose* to increase the reader's understanding.

The immediate response to AICC as a serious and remarkable undertaking may well be considered a hopeful sign that the stereotype of the Indian as the "vanishing American" can be changed. Perhaps the public at large can be made to realize that Indians are not just going to disappear like the extinct carrier pigeon, regrettably but inevitably destroyed by Progress. Premature optimism is, of course, dangerous. Communication of ideas and changing of entrenched opinions take time. Since this is true of Indians as well as Whites, it is likely that Indians are going to be plagued by inner dissent and the public will continue in ignorance for some while to come. Nevertheless, AICC makes two matters eminently clear: first, programs forced upon Indians can no longer be justified, in default of convincing evidence to the contrary, by the opinion that Indians are incapable of planning for themselves in their own best interests; second, the success of AICC in demonstrating that Indians can plan for themselves was not a matter of chance. It was due to the application of sound, general principles of letting people work out their own problems. These principles do not apply only to Indians, but the format in which the principles operate must be appropriate to the situation of the group.

References Cited

- BROOKINGS INSTITUTION, Washington, D. C. Institute for Government research. 1928. *The problem of Indian administration: report of a survey made at the request of the Honorable Hubert Work, Secretary of the Interior*. Survey staff: Lewis Meriam, technical director. Baltimore, Maryland: the Johns Hopkins University Press.
- GEARING, FREDERICK OSMOND, et al. (Eds.). 1960. *Documentary History of the Fox Project*, directed by Sol Tax. Chicago: University of Chicago Press.
- TAX, SOL. 1952. Action Anthropology. *America Indigena* 12:103-09.
- TAX, SOL. 1959. Action Anthropology. *Journal of Social Research* 2:1-9.

DISCUSSION AND CRITICISM

REJOINDER TO FORTES

[THROUGH AN EDITORIAL oversight, a letter from Meyer Fortes was printed in the October 1961 issue of CA (p. 398) without G. P. Murdock's being given an opportunity to reply. His rejoinder, therefore, is printed in this issue.]

I yield to no man, including Leach and Freeman, in my admiration of the contributions of Meyer Fortes to the analysis of social structure, particularly his "highly germinal essay" (Fortes 1949). It was with sorrow, therefore, that, in *Social Structure in Southeast Asia*, I noted elements of retrogression and confusion in his recent article in *Man* (vol. 59, 1959). And it is with sorrow that I find, from his note in CA (vol. 2, no. 4), that he feels that I misrepresented his position. Careful rereading of what both of us have written does not convince me that I have done him an injustice. His note, it seems to me, not only does not clear up the confusions which I cited, but it introduces new ones. In particular, on what basis can Fortes conclude that my denial that the bilateral "kindred" is a descent group is identical with his insistence that the ambilateral "ramage" of Firth is not a descent group unless it is endogamous? The kindred and the ramage are completely different phenomena. I follow Rivers, Freeman, Leach, Goodenough, and Davenport in the definition of the former, and Firth, Davenport, and Goodenough in the definition of the latter.

It is apparent to me that the seeming differences between Fortes and myself can be resolved only by assuming that we start with differing definitions, which perhaps neither of us has made clear. To me, a "descent group" is a group of kinsmen united by descent from a common ancestor, however the links from that ancestor may be traced and regardless of the extent to which its members intermarry with one another. A Maori *hapu* or any other ramage, whether endogamous or exogamous, is thus a descent group by definition. The denial of this by Fortes makes no more sense to me than does his insistence that a Jewish ghetto or an Indian caste, if endogamous, meets the definition of a descent group. Is he perhaps thinking of a descent group in biological terms—

a gene pool as it were—whereas I am using the term in a strictly sociological sense?

Again, Fortes must be using a private definition of "bilateral." I employ this term in its long-standing American meaning of "related on either the father's or the mother's side," as in "bilateral kindred." Fortes must mean "related on both sides," else he would not insist that a ramage is not a descent group unless it is endogamous.

The profession must decide which of our definitions are the more useful and productive. I can only say that I find myself completely at home with the concepts and definitions of such analysts of social structure as Davenport, Eggan, Firth, Freeman, Goodenough, and Leach, but feel in an uncomfortable straightjacket when I "try on for size" the recent conceptual framework of Fortes.

GEORGE P. MURDOCK
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DIFFUSION AND INVENTION

Edmonson's article on "Neolithic Diffusion Rates" [April 1961, pp. 71-102] was rightly stimulating, whether to wrath, to scorn, or to pondering. For each mood one had to produce evidence, and so it was a good choice for CA. Amongst various ideas that came to me while reading it were two I might mention. (1) There must surely be enough data available now for the study of the spread of cattle to be undertaken on similar lines to that of pottery. (2) Any discussion on the spread of traits from a single centre at once makes one think of the same idea arising in more than one place; an old controversy, I know. The way in which such pottery-making might have arisen in East/Central Africa was illustrated for me whilst I was in the field last year. I found what was, to all intents and purposes, a terra-cotta tree branch, complete with knots. Its origin was clearly thus: the branch falls to the ground; its inner wood is completely eaten away by termites and replaced by a cast in termite-masticated clay; a forest-fire sweeps the area, baking the clay hard and destroying the bark; a terra-cotta branch remains. This process, observed 8,000 years ago by an intelligent inhabitant of Gamble's Cave, might have

set him to making something on the same lines: pottery has started.

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CHARISMA AND PEYOTISM

Weston LaBarre's excellent review article, "Twenty Years of Peyote Studies," (CA, January, 1960) omitted an important aspect of the study of any religious movement: the "routinization of Charisma" or enthusiasm. This aspect has rarely been studied by anthropologists, who have generally emphasized the origin of the religion; its historical roots, whether Indian or White; and the psychological traits of its adherents. The internal structure of the Peyotist grouping and its development over the years are, by and large, neglected.

An analysis of the Peyote Way would show that the religion centers about an elaborate ceremony, in which Peyote is worshipped as a supernatural being, and consumed as a vision-inducing drug. The purpose is to heal a sick person or to produce religious visions. The event is communal, but the goals are individualistic. Ethical and spiritual purity is stressed. The individual must be pure if Peyote is to have the desired effects; otherwise it may act as a poison and cause harm. Vomiting produced by the drug is interpreted as cleansing the individual. The vision is intimately connected with the sacrament.

It is conceivable that control of the rite, the agency by which religious goals are attained, might become the property of a small group of knowledgeable specialists and of those who might monopolize the Peyote supply. This has seldom happened among the adherents of the standard Kiowa-Comanche rite, although in some groups various individuals play more central roles. Generally, anthropologists consider this aspect of leadership a part of the egalitarianism of Plains and other North American Indian societies. It is argued here that this feature of the Peyote religion should be considered from the historical viewpoint and analyzed in terms of the inner structure of the religious grouping.

The loose ideological and organizational structure of Peyotism outside the actual framework of the ceremonial setting indicates that it is different from the tightly organized sects and churches of the Weber-Tröeltsch typology. The locus of salvation (*viz.* goals of healing and vision) is centered upon the present. Ideas of the afterlife and the apocalypse are vague, and do not play a major role in Peyote ideology. More important than the millenium or the life after death are such elements as

healing and purity of mind and body. The vision which may occur during the sacrament gives the individual justification (or condemnation) for his actions. This experience is in itself a reality within the everyday reality and can be reproduced from time to time. It is doubtful that subsequent visions are experienced as intensely as is the first vision. D'Azevedo (personal communication) has pointed out that many individuals do not have visions during the ceremonial consumption of Peyote, although their perceptions may be heightened.

The fact that goal-orientation is immediate and individualistic makes loose organization understandable, even if the historical North American background is ignored for the moment. The strength of leadership in maintaining the cult or possible familial monopolies of the cult deserves consideration in future studies to see whether the religious grouping becomes exclusive. By ideology, it should be open to those desiring purification: but does it remain so? The local influence of the Native American Church, as a national organization for defense and anti-defamation, also deserves consideration. Does such a national organization stimulate the growth of a bureaucracy?

It is my hypothesis that the Peyote Way will tend to remain non-hierarchical and will continue to emphasize the sacrament. Ideologically it will be open, but it will be a "familial monopoly," depending, in part, upon the opposition to it. If the opposition is strong, tendencies toward exclusion may be strengthened. These conclusions should be tested by field studies of Peyotist groups.

Parenthetically, it might be noted that the Peyote Way, the Ghost Dance, and other North American enthusiastic religious movements bear some resemblance to "anti-witchcraft" movements among such African peoples as the Tiv (Bohannan 1958).

References

- BOHANNAN, PAUL. 1958. Extra-processual events in Tiv political institutions. *American Anthropologist* 60:1-12.
- D'AZEVEDO, WARREN. Personal communication.
- LA BARRE, WESTON. 1960. Twenty years of Peyote studies. *Current Anthropology* 1:45-60.
- TRÖLTSCH, E. 1931. The social teachings of the Christian churches. London.
- WEBER, MAX. 1946. "The social psychology of world religions," in *From Max Weber*, edited by H. GERTH and C. W. MILLS. London: Oxford University Press.
- WALTER P. ZENNER
New York

Offered

► *Klamath Personalities: Ten Rorschach Case Studies*, by J. A. Clifton and David Levine, will be distributed without charge to interested specialists in culture and personality, as long as copies are available. Copies may be obtained by writing: J. A. CLIFTON, Tri-Ethnic Research Project, University of Colorado, Box 626, Ignacio, Colorado, U.S.A.

Serial Publications

► *Ethnology: An International Journal of Cultural and Social Anthropology*, a new quarterly journal, will begin publication in January 1962. It is designed to fill a long-standing need, since ethnology, though the largest subfield of anthropology, has hitherto lacked a specialized journal in the United States. Edited by George P. Murdock, with the assistance of his colleagues in the Department of Anthropology at the University of Pittsburgh and of an International Editorial Board with representatives from about 20 countries, *Ethnology* will publish only articles, not news items or reviews.

Contributions of high quality, ranging from 2 to 50 printed pages, are solicited from scholars of any country and on any aspect of cultural anthropology, including emergent interests as well as such established specialties as cultural change, social organization, and culture and personality. No particular approach—descriptive, analytical, distributional, typological, historical, or comparative—will be favored or discriminated against; but theoretical or methodological discussions, to be acceptable, must be specifically related to some body of substantive data.

Manuscripts should be submitted in double-spaced typescript and addressed to the DEPARTMENT OF ANTHROPOLOGY, University of Pittsburgh, Pittsburgh 13, Pennsylvania, U.S.A. Instead of footnotes, references should be inserted in parentheses in the text, with a bibliography of sources cited at the end of the article. Authors will receive 50 reprints gratis.

The subscription price will be U.S.A. \$5.00 or equivalent per annum; exchanges will be welcomed with institutions in any country which publish journals or series in anthropology or related sciences.

► *History and Theory*, an international journal devoted to the philosophy of history and historiography, is published

by Mouton and Co. under the editorship of George H. Nadel. Its editorial committee consists of 8 members: Raymond Aron (France), Isaiah Berlin (Great Britain), Crane Brinton (U.S.A.), Pieter Geyl (Netherlands), Sidney Hook (U.S.A.), Maurice Mandelbaum (U.S.A.), W. H. Walsh (Great Britain), and Morton White (U.S.A.). Founded to promote the study and application of the philosophy of history, *History and Theory* publishes monographs, reviews, essays, exchanges, comments, and bibliographies in four areas: theory of history; historiography; method of history; and related disciplines. To accommodate a variety of contributions irrespective of length, individual issues vary in size and are mailed to subscribers as they appear and not according to a quarterly schedule. An accumulation of individual issues which reaches book-length constitutes a volume.

Editorial correspondence should be addressed to: EDITOR, *History and Theory*, Box 99, Cambridge 38, Mass., U.S.A. Subscriptions may be entered with local booksellers or with the publishers, MOUTON AND CO., The Hague, Netherlands. The price of an annual subscription is U.S.A. \$5.00 or equivalent.

► *Abstracts of New World Archaeology, Volume One—1959* begins a new series (CA March, 1960, p. 147). This first volume includes 676 entries, all but a few with abstracts of their contents. Titles are grouped geographically, and an index of authors is provided. Most of the titles are of publications that appeared in 1959.

Where possible, abstracts appearing with the original publication are reprinted, but the majority of the abstracts have been prepared by a group of about 40 co-operating scholars in 7 countries. Volume 2, abstracting 1960 titles, is now in preparation, and its distribution is planned for early autumn, 1961.

It is expected that this series will be a valuable research tool, particularly for scholars with limited library facilities. In order to make each volume as complete as possible, authors are urged to send separates, reprints, or titles (with full bibliographical details) to the Editor: RICHARD B. WOODBURY, Department of Anthropology, University of Arizona, Tucson, Arizona, U.S.A. Any 1959 titles missed in Vol. 1 will be appreciated.

The volume, which is distributed to all individual and institutional members of the Society, is also available to others for \$3.50 from: THE SOCIETY FOR AMERICAN ARCHAEOLOGY, 1530 P STREET, N.W., Washington 5, D.C., U.S.A.

Journal Contents

In this section we list the main articles in widely circulated journals, most of the contributors to which are Associates in CURRENT ANTHROPOLOGY. The selection of journals which follows reflects the material on hand at the time of publication, and will be expanded in future issues. Editors of these and similar publications are requested to send tables of contents even before forthcoming issues are completed. Sources for further information, when known, are indicated in the footnotes.

Acta Linguistica

Postafók 440, Budapest 62, Hungary.

GY. LAKÓ: Pál Hunfalvy.

W. STEINITZ: Etymologische Beiträge. I. Zu Hunfalvy-Vologodskij's Ostjakischem Wörterbuch.

L. LIGETI: A propos des éléments "altaïques" de la langue hongrois.

B. A. SEREBRINNEKOV: Ungeklärte Fragen des Geschichte der permischen Sprachen.

K. MÖLLAY: Zur Chronologie deutscher Ortsnamensarten im mittelalterlichen Westungarn.

L. DEME: Disputed Aspects of Phonetics.

K. RADANOVICS: The Origin of the People's name 'Chanti.'

M. K. PALLÓ: Zum Problem der ungarischen Wortgruppe *égyház, igyfondi; ügyűsa; imnep, üdvöz, imnepet ülni*.

L. RÁSONYI: L'origine du nom *szekely* (sicule).

American Anthropologist

American Anthropological Association, 1530 P. Street, N.W., Washington, D.C., U.S.A.

Volume 67, No. 3, June 1961

MARGARET MEAD: Anthropology Among the Sciences.

PEDRO CARRASCO: The Civil-Religious Hierarchy in Mesoamerican Communities: Pre-Spanish Background and Colonial Development.

ELAINE CUMMING and DAVID M. SCHNEIDER: Sibling Solidarity: A Property of American Kinship.

EDWARD M. BRUNER: Urbanization and Ethnic Identity in North Sumatra.

ROBERT J. SMITH: The Japanese Rural Community: Norms, Sanctions, and Ostacism.

THOMAS O. BEIDELMAN: Beer Drinking and Cattle Theft in Ukaguru: Intertribal Relations in a Tanganyika Chiefdom.

RICHARD B. WOODBURY: A Reappraisal of Hohokam Irrigation.

American Antiquity

Society for American Archaeology, 1530 P. Street, N.W., Washington 5, D.C., U.S.A.

Vol. 2 · No. 5 · December 1961

Volume 26, No. 3, Part 1, January 1961

WILLIAM G. HAAG: The Archaic of the Lower Mississippi Valley.

JOHN HOWLAND ROWE: Stratigraphy and Seriation.

ROBERT L. RANDB: Elaboration and Invention in Ceramic Traditions.

ROBERT M. ADAMS: Changing Patterns of Territorial Organization in the Central Highlands of Chiapas, Mexico.

HAL EBERHART: The Cogged Stones of Southern California.

RENÉ MILLON and BRUCE DREWITT: Earlier Structures within the Pyramid of the Sun at Teotihuacán.

CARLING MALOUF: The Tipi Rings of the High Plains.

GORDON L. GROSSCUP: A Sequence of Figurines from West Mexico.

Volume 26, No. 4, April 1961

HUGH C. CUTLER and THOMAS W. WHITAKER: History and Distribution of the Cultivated Cucurbits in the Americas.

A. P. OKLADNIKOV: The Palcolithic of Trans-Baikal.

STEPHAN F. DE BORHIEGY: Miniature Mushroom Stones from Guatemala.

MICHAEL D. COE and CLAUDE F. BAUDEZ: The Zoned Bichrome Period in Northwestern Costa Rica.

RENÉ MILLON and JAMES A. BENNYHOFF: A Long Architectural Sequence at Teotihuacán.

INA VANSTAN: Miniature Peruvian Shirts with Horizontal Neck Openings.

Anthropologischer Anzeiger

E. Schweizerbart'sche Verlagsbuchhandlung (Nägele u. Obermiller) Johannesstr. 3/1, Stuttgart W., Germany.

Volume 24, No. 4, 1961

ALBERTO C. BLANC: Leucal I. Der erster fossile Fund eines Neandertalers von Salento (südliches Apulien, Italien).

G. G. WENDT, K. SOLTH, and H. J. LANDZETTEL: Kinderzahl, Erkrankungsalter und Sterblichkeit bei der Huntington'schen Chorea.

HANS FLEISCHHACKER: Zur "multifaktoriellen" Vererbung normaler metrischer und morphologischer Merkmale beim Menschen.

Anthropos

Imprimerie Saint-Paul, Fribourg, Switzerland

Volume 56, Nos. 1-2, 1961

W. KOPPERS: Die ältesten Formen des Staates und das Verständlichwerden des hohen Alters der Menschheit in universalgeschichtlicher (ethnologischer und prähistorischer) Schau.

J. HUPPERTZ: Untersuchungen über die Anfänge der Haustierzucht unter besonderer Berücksichtigung der Pferdezucht.

J. LOWENSTEIN: Rainbow and Serpent.
H. LOOFS: Zur Frage des Megalithentums im Südöstlichen Hinterindien.

Archaeology

Archaeological Institute of America, 5 Washington Square North, New York 3, N.Y., U.S.A.

Volume 14, No. 3, Autumn 1961

WOLFGANG HABERLAND: Two Shaman Graves from Central America.

ARTHUR KAHN: Apollonia: City of Statues.

DMITRY SHELOV and NIKOLAI MERPERT: Soviet Archaeological Expeditions in 1961.

JAMES F. ROSS and LAWRENCE THOMBS: Three Campaigns at Biblical Shechem.

HALSTED B. VANDERPOEL: The Frieze of the House of Meleager at Pompeii.

ARNOLD H. WEISS: The Roman Walls of Barcelona.

ALFONS WOTSCHITZKY: Ephesus: Past, Present and Future of an Ancient Metropolis.

Asian Perspectives

Volume 4, Nos. 1-2, Summer-Winter 1960

DAVID CHASE: A Limited Archaeological Survey of the Han River Valley in Central Korea.

CHESTER S. CHARD: Neolithic Archaeology in North Korea.

A. P. OKLADNIKOV: Palaeolithic Sites in Trans-Baikal, translated by Chester S. Chard.

S. G. DAVIS and MARY TREGEAR: Man Sok Tsui, Archaeological Site 30, Lantau Island, Hong Kong.

Cahier d'Études Africaines

Centre d'Études Africaines, Ecole Pratique des Hautes Études, VI Section, 20, rue de la Baume, Paris 8, France.

No. 6, October 1961

PIERRE ALEXANDRE: Problèmes linguistiques des États négro-africains à l'heure de l'indépendance.

MARIE-JOSÉ TUBIANA: Le marché de Hiliba: Moutons, sel et contrebande.

JAMES W. FERNÁNDEZ: Christian Acculturation and Fang Witchcraft.

J. L. VINCKE: Systématique des termes de parenté.

Człowiek w Czasie i Przestrzeni (Man in Time and Space)

Panstwowe Wydawnictwo Naukowe, UL Miodowa 10, Warsaw, Poland.

Volume 3, No. 4, 1960

TADFUSZ DZIERZYKRAY-ROGALSKI: Anthropological investigations of Jaćwings.

TAMARA SAMKOWICZ-JELISIEJEW: On the *sternum orangoideum*.

NAPOLEON WOLAŃSKI: On the causes of the increasing human stature.

ALEKSANDER LECH GODLEWSKI: Discussion of the causes of changes that happened in the anthropological and somatic structure of certain chosen populations.

ELZBIETA PROMIŃSKI: Anthropological investigations of the bone remnants from the cremations burials.

Volume 4, No. 1, 1961

DANUTA GOSTYŃSKA: Plastic reconstructions of the Neanderthal Man.

JANINA WOLSKA: Protozoological aspects of human filogenesis.

TADFUSZ DZIERZYKRAY-ROGALSKI: The present state of Anthropology in Czechoslovakia.

JAROSLAV SUCHÝ: Anthropological Laboratory of the Pedagogical Institute in Praha.

BARBARA PERZYNA: Impressions from a journey to Czechoslovakian centres of Anthropology and Medicine.

TADEUSZ DZIERZYKRAY-ROGALSKI: Anthropology in Japan.

Ethnos

Statens Etnografiska Museum, Stockholm NO, Sweden.

Volume 26, Nos. 1-2, 1961

GUNNAR JARRING: A Note on Shamanism in Eastern Turkistan.

G. MONTELL: Siberian Exhibition.

V. DUSENBERRY: The Significance of the Sacred Pipes.

RUSSELL H. MITCHELL: Preliminary Report on Wooden Artifacts.

S. RYDEN: Complementary Notes on Pre-Tiahuanaco Site.

G. POLYKRATES: Anthropologie, Ethnographie und Sprachforschung der Kashuina Indianer.

A. HULTKRANTZ: Bachofen and the Mother Goddess.

G. NYKVIST: Round the World by Primitive Craft.

Etnografia Polska (Polish Ethnography)

Institute of the History of Material Culture, Polish Academy of Sciences, Warsaw, Poland.

Volume 4, 1961

KAZIMIERZ DOBROWOLSKI: Studies on the Theory of Folk Culture (English summary).

MARIA DEMBIŃSKA: Food Consumption in Medieval Poland (Russian summary).

JADWIGA KLIMASZEWSKA: Magical Prohibitions Connected with the Traditional Ceremonial Year in Poland (English summary).

WITOLD ARMON and MIKOLAJ WITASZEWSKI: Polish Explorer of the Jakuts.

ANNA KUTREZEBA-POJNAROWA: The Atlas of the Polish Folk Culture as one of the Forms of Ethnographic Research Work (German summary).

L'Homme

Editions Mouton & Co., 68, quai des Orfèvres, Paris 1, France

Vol. 1, No. 1, January-April, 1961

ANDRÉ G. HAUDRICOURT: Richesse en phonèmes et richesse en locuteurs.

LOUIS DUMONT: Les mariages Nayar comme faits indiens.

DENISE PAULME: Littérature orale et comportements sociaux en Afrique noire.

JACQUES BERQUE: Expression et signification dans la vie arabe.

JACQUES GERNET: L'âge du fer en Chine.

GILLES SAUTTER: L'étude régionale: réflexions sur la formule monographique en géographie humaine.

Human Biology

Wayne State University Press, Detroit, Michigan, U.S.A.

Volume 33, No. 1, February 1961

RICHARD G. SNYDER: The Sex Ratio of Offspring of Pilots of High Performance Military Aircraft.

JACK B. BRESLER: Effect of ABO-Rh Interaction on Infant Hemoglobin.

J. B. WATSON, V. ZIGAS, OLGA KOOPITZOFF and R. T. WALSH: The Blood Groups of Natives in Kainantu, New Guinea.

H. M. C. LUYTX and BETTY L. MURRAY: Duration of Illness.

FRANCIS EDWARD JOHNSON: Sequence of Epiphyseal Union in a Prehistoric Kentucky Population from Indian Knoll.

Volume 33, No. 2, May 1961

STEVEN POLGAR: Evolution and the Thermodynamic Imperative.

FRANCISCO M. SALZANO: Studies on Cain-gang Indians.

MORRIS GOODMAN: The Role of Immunological Differences in the Phyletic Development of Human Behavior.

PHILIP J. CLARK, STEVEN G. VANDENBERG and CHARLES H. PROCTOR: On the Relationship of Scores on Certain Psychological Tests with a Number of Anthropometric Characters and Birth Order in Twins.

A. O. OETTLÉ and J. HIGGINSON: The Age of Menarche in South African Bantu (Negro) Girls.

Human Organization

Society for Applied Anthropology, New York State School of Industrial and Labor Relations, Ithaca, N.Y., U.S.A.

Volume 20, No. 2, Summer 1961

MICHEL CROZIER: Human Relations at the Management Level.

SYDNEY H. CROOG: Ethnic Origins and Responses to a Health Questionnaire.

ELIHU KATZ: The Social Itinerary of Technical Change.

J. L. FISCHER: The Japanese Schools for the Natives of Truk.

HOWARD E. FREEMAN and OZZIE G. SIMMONS: Consensus and Coalition in the Release of Mental Patients.

ERWIN L. LINN: Tolerance of Deviant Behavior.

International Journal of American Linguistics

Waverly Press Inc., Mt. Royal and Guilford Avenues, Baltimore 2, Maryland, U.S.A.

Volume 27, No. 3, July 1961

M. S. BEELER: Northern Costanoan.

FRED W. ADLER: A Bibliographical Checklist of Chimakuan, Kutenai, Ritwan, Salishan, and Wakashan Linguistics.

GEORGE L. TRAGER: Taos IV: Morphemics, Syntax, Semology in Nouns and in Pronominal Reference.

WALLACE L. CHAFE: Seneca Morphology VII: Irregularities and Variants.

DESMOND DERBYSHIRE: Hishkaryana (Carib) Syntax Structure: II.

FRANK E. ROBBINS: Quioitepec Chinantec Syllable Patterning.

Volume 27, No. 4, October 1961

C. F. VOEGELIN: Typology of Density Ranges II: Contrastive and Non-Contrastive Syntax.

BRUCE R. MOORE: A Statistical Morpho-Syntactic Typology Study of Colorado (Chibcha).

D. L. OLMSTED: Substitution and Frequency-Estimation in Linguistic Analysis.

WALLACE L. CHAFE: Seneca Morphology VIII: Charts, Indexes, Corrections and Additions.

LORAIN I. BRIDGEMAN: Kaiwa (Guarani) Phonology.

PERRY N. PRIEST, ANNE M. PRIEST, and JOSEPH E. GRIMES: Simultaneous Orderings in Siriono (Guarani).

HARRY ROSBOLTONI: Different-Level Tense Markers in Guarani.

Japanese Journal of Ethnology (Minzokugaku-Kenkyu)

Editor: Eiichiro Ishida, Japanese Society of Ethnology, 132 Shimohôya, Hôyamachi, Tokyo, Japan.

Volume 25, Nos. 1-2

KENJIRO ICHIKAWA: Shang, Wu-yueh and the Cultures of the Pacific.

YOSHISHIGE HAYASHI: Observation on Ainu Techniques of Cooking Agricultural Products.

YUKO YAMAMOTO: Folktales of the Orocco.

TAKAO NAKAMURA and FUMI KUWANO: Some Observations on *Kijiya*.

Volume 25, No. 3

KINJI IMANISHI: The Origin of Human Family—A Primatological Approach.

KIYOKO SEGAWA: Notes on Ainu Ethnography, I.

Journal of the Polynesian Society

The Polynesian Society, Box 5195, Wellington, New Zealand.

Volume 70, March 1961

R. GERARD WARD: A Note on Population Movements in the Cook Islands.

ANTONY HOOPER: The Migration of Cook Islanders to New Zealand.

IAN FAIRBAIRN: Samoan Migration to New Zealand.

GABRIELLE MAXWELL: Some Demographic Indications of Population Movement Among New Zealand Maoris.

IAN POOL: Maoris in Auckland. A Population Study.

H. E. MAUDE: Post-Spanish Discoveries in the Central Pacific.

WARD H. GOODENOUGH: Migration Implied by Relationships of New Britain Dialects to Central Pacific Languages.

The Journal of the Royal Anthropological Institute of Great Britain and Northern Ireland

Royal Anthropological Institute, 21 Bedford Square, London, WC1, England.

Volume 91, Part 2, December, 1961

AUDREY I. RICHARDS: African Kings and their Royal Relatives. (Presidential Address)

M. FORTES: Pietas in Ancestor Worship. (Henry Myers Lecture)

J. D. FREEMAN: The Concept of the Kindred. (Curl Bequest Prize Essay)

N. J. B. PLOMLEY: Tasmanian Aboriginal Material in Collections in Europe.

B. M. FAGAN: A Collection of Nineteenth Century Soli Ironwork from the Lusaka Area, Northern Rhodesia.

G. W. B. HUNTINGFORD: The Distribution of Certain Culture Elements in East Africa.

G. B. MILNER: The Samoan Vocabulary of Respect.

DON BROTHWELL: The Palaeopathology of Early British Man: an Essay on the Problems of Diagnosis and Analysis. (Curl Bequest Prize Essay)

S. R. DAS, N. KUMAR, P. N. BHATARCHARJEE and D. B. SASTRY: Blood Groups (ABO M-N and Rh), ABH Secretion, Sickle-cell, PTC Taste, and Colour Blindness in the Mahar of Nagpur.

(Continued on page 509)

Publications Received

The Editor wishes to acknowledge with thanks the receipt of the publications listed below. To make our listings more accurate and serviceable, each publication sent to the Editor should include on the cover or title page complete information (with date, city, etc.), translated into English and, where necessary, transliterated into Roman script.

Serials

- Annual Report of the Department of Antiquities of Tanganyika for the year 1959.* 16 pp. Dar Es Salaam, 1960.
- Antiquités Nationales et Internationales* (1960), Fasc. 3-4: 1-96. Saint-Germain-en-Laye: Société des Amis du Musée des Antiquités Nationales.
- Archaeometry* (1960) 3. (Bulletin of the Research Laboratory for Archaeology and the History of Art, Oxford University.)
- Arts et traditions populaires* (1959) 7, Nos. 3-4. Paris: Editions des Quatre Jéudis.
- Asian Culture* (1960) 2, Nos. 1-2. Saigon: Vietnamese Association for Asian Cultural Relations.
- Boletín del Instituto Indigenista Nacional*, Vol. 2 (1956); Vol. 3 (1957). Guatemala A.C.: Ministerio de Educación Pública.
- Boletín del Instituto de Antropología* (1960). Medellín, Colombia: Universidad de Antioquia.
- Curator* 3, No. 4: 327-56. New York: American Museum of Natural History.
- Dacia* (Revue d'Archéologie et d'Histoire Ancienne) 3 (1959): 611 pp. Bucharest: Institute d'Archéologie.
- Estudios de Cultura Nahuatl* (1959) 1. Mexico D.F.: Seminario de Cultura Nahuatl, Instituto de Historia, Universidad Nacional de Mexico.
- Ethnographica* (1959). 307 pp. (Yearbook of the Moravian Museum, Brno, Czechoslovakia.)
- Ethnohistory* (1961) 8, No. 2. Bloomington, Indiana: Research Center for Anthropology, Folklore, and Linguistics.
- Folk-Liv* (1959) 23: 2: 66 pp. Stockholm: Kungl. Gustav Adolfs Akademien.
- Jahrbuch des Museums für Völkerkunde zu Leipzig* 18. Berlin: Akademie-Verlag, 1961.
- Journal of Conflict Resolution*, (1961) 5, No. 1, 114 pp.
- Kultuurpatronen* (1960) 2, 104 pp. (Bulletin of the Ethnographical Museum in Delft.)
- Mémoires de l'Institut Scientifique de Madagascar* (1959) 5, 266 pp. Office de la Recherche Scientifique Outre-Mer. Paris: Pierre André.
- Műveltség és Hagymány (Kultur und Tradition)* (Jahrbuch der Ethnologisches Institut an der Universität Debrecen.) 359 pp. Budapest: Tankönyvkiadó, 1960.
- Prehled Vyzkumu* (1959): 179 pp. Brno, Czechoslovakia.
- Revista Colombiana de Folclor* (1960) 2, No. 4 (205 pp.), No. 5 (197 pp.). Bogota: Instituto Colombiano de Antropología.
- Sibirium* (1960) 5: 302 pp. Centro di Studi Preistorici ed Archeologici, Varese Musei

Civici di Villa Maraballo. Varese, Italy. *Social Anthropologist* 3, No. 1: 93 pp. Tokyo: Social Anthropology Circle, Tokyo Metropolitan University.

Books and Offprints

- ABD EL-RASOUL, KAWTHAR, Subsidiary incomes and secondary economies in Mishla complex. *Bull. de la Société de Géographie d'Égypte* (1960) 33:311-32.
- ALCINA, J., Bibliografía básica de arqueología Americana. 124 pp., mimeo. *Publications of Seminario de Antropología Americana* 1. Seville, 1960.
- ALEKSEEF, V. P., Neolithic skull from Krasnojarsk (southern mixture in population of Altai-Sajan region). *Kratkije soobshchenija instituta ethnographii* (1960) 34:79-85.
- ALEKSEEF, V. P., Materials on the palaeo-anthropology of Western Jura. *Publication of Jura archaeological and ethnographical expedition* 1:284-312. Moscow: Publishing House of Acad. of Scis. of U.S.S.R., 1960.
- ALEKSEEF, V. P., *Some questions concerning hand development in human evolution (the place of Kiik-Koba remains among other neanderthalian finds)*. *Antropologičeskij sbornik* 2. Moscow: Publishing House of Acad. of Scis. of U.S.S.R., 1960.
- AMYOT, JACQUES, S. J., *The Chinese community of Manila: A Study of adaptation of Chinese families to the Philippine environment*. 181 pp. Chicago: Philippine Studies Program, University of Chicago, 1960.
- ANDREWS, E. WYLLIS, *Preliminary report on the 1959-60 field season, National Geographic Society-Tulane University DZibichaltun Program*. Middle American Research Institute Miscellaneous Series No. 11. New Orleans.
- BARTÓK, BÉLA, *Slovenské Ľudové Piesne* (Slowakische Volkslieder), Vol. I. 752 pp. Bratislava: Academia Scientiarum Slovaca, 1959.
- BECHER, HANS, *Die Surára und Pakidái: zwei Yanonámi-Stämme in Nordwestbrasilien*. 138 pp., 16 plates. Hamburg: Mitteilungen aus dem Museum für Völkerkunde 26, 1960.
- Behavioral approaches to accident research*. Papers presented at a conference on research in accident prevention sponsored by the Association for the Aid of Crippled Children, held at West Point, N.Y., U.S.A., October 20 and 21, 1960. New York: Ganis & Harris.
- BLANCHARD, JACQUES, *Les Industries Préhistoriques de Bellême (Orne)*. *Bulletin de la Société Préhistorique Française* 61, Nos. 9-10, 1959.
- BORISKOVSĀJ, P. J., Abitazioni paleolitiche nel territorio dell' U.R.S.S. e loro paralleli etnografici. *Rivista di Scienze Preistoriche* (1958) 13: Fasc. 1-4:55-69.
- BORISKOVSĀJ, P. J., Die jungpaläolithische Siedlung mit dem Begräbnis eines Cromagnonmenschens in Kostjenki II am Don (UdSSR). *Anthropozoikum* (1958) 8: 17-22.
- BORISKOVSĀJ, P. J. *Novejšije Dostizheniia*

- Sovetskoi Arkheologii* (Latest developments in Soviet Archaeology). 44 pp. Leningrad: 1959.
- BUGYI, BLASIUS, Röntgenologische Beitrag zur Bestimmung des Ernährungszustandes. *Zeitschrift für Ernährungswissenschaft* (1961) 2, Seite 23:23-26.
- BUGYI, BLASIUS, Röntgenuntersuchungen zur Beurteilung der Skelettreife älterer Schulkinder. *Artzliche Jugendkunde* (1960) 52, Heft 11-12:329-40.
- BUNAK, V. V., Y. Y. ROGINSKIY, G. F. DEBETS, M. G. LEVIN, and N. N. ČEBOKSAROV, *Contemporary raciology and racism* (a symposium). 146 pp. Indiana University Research Center in Anthropology, Folklore, and Linguistics, Publication 17, 1961.
- BURGFALLER, ERNST, *Felsbilder und Inschriften im Toten Gebirge in Oberösterreich*. 46 pp., 8 plates. Linz: Institut für Landeskunde von Oberösterreich, 1961.
- BURSENS, H., *Enkele Zand-maskers uit Uelc. Congo-Tervuren* (1960) 6, No. 4: 101-08.
- BUTLER, B. ROBERT, *The Old Cordilleran Culture in the Pacific Northwest*. 111 pp. Occasional papers of the Idaho State College Museum, No. 5. Pocatello: 1961.
- CAPPIERI, MARIO, L'analisi discriminante in antropologia. *Rivista Italiana di Economia Demografia e Statistica* (1960) 14, No. 3:1-25.
- CAPPIERI, MARIO, La popolazione preistorica della civiltà dell'Indo. *Revista di Scienze Preistoriche* (1959) 14, 1-4:123-74.
- CAPPIERI, MARIO, La popolazione preistorica dell'Iran. *Scienza e Tecnica* (1960) 4, No. 1:47-60.
- CAPPIERI, MARIO, *L'India preistorica*. 225 pp. Florence: Sansoni, 1960.
- CARRASCO, PEDRO, Pagan rituals and beliefs among the Chontal Indians of Oaxaca, Mexico. *University of California Anthropological Records* (1960) 20, 3:83-117.
- CHARD, CHESTER S. (Ed.), Japanese source materials on the archaeology of the Kurile Islands. 6 microcards. *Archives of Archaeology* No. 7. Madison, Wis.: University of Wisconsin Press.
- CLOSS, ALOIS, Gestaltkriterium und historisches Prinzip in der Völkerkunde. *Festschriften für Ehrenfels*. Darmstadt: 1961.
- COHEN, MARCEL, Quelques considérations sur le phénomène des verbes auxiliaires. *Studii si Cercetări Linguistice* (1960) 11, No. 3:433-442.
- COMAS, JUAN, *Pigmeos en America?* 49 pp. Cuadernos del Instituto de Historia, Serie Antropologica No. 9. Mexico D.F.: Universidad Nacional de Mexico.
- CŠERMAK, GÉZA, Aspects historiques de la technique des instruments de métal pour la pêche en Europe centrale. *Folk-Liv* (1959) 23:5-13.
- DE ARMAS CHITTY, J. A., *Tucupido: formation de un pueblo de llano*. 291 pp. Caracas: Imprento Universitaria, 1961.
- DE CARVALHO NETO, PAULO, *Folklore y Educacion*. 315 pp. Quito: Editorial Casa de la Cultura Ecuatoriana.
- DE LAGUNA, GRACE A., *The Lebenswelt and the Cultural World*. *Journal of Philosophy* 57, No. 25:777-91.
- DIAMOND, STANLEY (Ed.), *Culture in history: Essays in honor of Paul Radin*. 1014 pp. Published for Brandeis University by Columbia University Press, New York, 1960.
- DUBRUL, E. LLOYD, Structural evidence in the brain for a theory of the evolution of behavior. *Perspectives in Biology and Medicine* 4, No. 1:40-57.
- DUBRUL, E. LLOYD, and CHARLES A. REED, Skeletal evidence of speech? *American Journal of Physical Anthropology* 18, No. 2:153-56.

- DI BRUL, E. LLOYD and DANIEL M. LASKIN, Preadaptive potentiality of the mammalian skull. *Anatomical Record* 138, No. 3:345.
- DUFFIELD, LATHEL F., and EDWARD B. JELKS: *The Pearson Site*. 83 pp. University of Texas. Dept. of Anthropology, Archaeology Series No. 4, 1961.
- DUNNING, R. W., Differentiation of status in subsistence level societies. *Transactions of the Royal Society of Canada* 54, Ser. 3, Sect. 2:25-32.
- DUSTIN, C. BURTON, *Peoyotism and New Mexico*. 51 pp. Santa Fe: Vergara Printing Co., 1960.
- FILD, HENRY, et al., *North Arabian desert archaeological survey, 1925-50*. 224 pp., 85 plates. Papers of the Peabody Museum of Archaeology and Ethnology, Harvard University, 65, No. 2. Cambridge: Peabody Museum, 1960.
- FORCE, ROLAND W. and MARYANNE FORCE, Keys to cultural understanding. *Science* (1961) 133, No. 3460:1202-06.
- FUCHS, HELMUTH, El aspecto teórico en el estudio de la producción alfarera. *Folia antropologica*, (1960) No. 1:37-87.
- FUCHS, HELMUTH, Investigaciones Etnológicas de Urgencia en Venezuela. *Bulletin of the International Committee on Urgent Anthropological and Ethnological Research* (1959) No. 2:36-49.
- FUCHS, PETER, *Die Völker der Südost-Sahara*. 254 pp., 16 plates. Vienna: Wilhelm Braumüller, 1961.
- GINES, HNO. and JOHANNES WILBERT, Una corta expedición a tierras Motilonas. *Memoria de la Sociedad de Ciencias Naturales La Salle* (1960) 20, No. 57: 159-74.
- GJESSING, GUTORM, Circumpolar social systems. *Acta Arctica* (1960) Fasc. 12:75-81.
- GJESSING, GUTORM, *The disintegration of the village organization of the Sea-Saames (Sea-Lapps); an hypothesis*. 25 pp. Oslo: Norske Videnskaps-Akademi i Oslo, 1960.
- GUMMEL, HANS, "Geschichte der Westerman-Funde im Morgenstern-Museum" in *9 Beiheft zum Atlas der Urgeschichte: 15-23*. Hamburg: Hamburgisches Museum für Völkerkunde und Vorgeschichte, 1960.
- GUMMEL, HANS, *Hermann Allmeyers und die Altertumsforschung*. 55 pp. Festschrift zur Wiederöffnung des Morgensternmuseums. Bremen: Nordwestdeutscher Verlag Ditzen & Co., 1961.
- GUNDA, BELA, Kulturstörungen und Gesellschaftliche Faktoren. *Acta Universitatis Debreceniensis de Ludovico Kossuth Nominatae* 6, No. 1:171-81. Debrecen: 1960.
- HAMPATE BA, A. and G. DIETERLEN, *Koumen: Texte initiatique des Pasteurs Peul*. 95 pp. Paris: Mouton & Co., 1961.
- HASELBERGER, HERTA, Wandermalerei, graviertes und modellierter Wandschmuck in den Savannen von Togo under Obervolta. *International Archives of Ethnography* 49, No. 2, 1960.
- HEATH, DWIGHT B., Land Reform in Bolivia. *Inter-American Economic Affairs* (1959) 12, No. 4:3-27.
- HEATH, DWIGHT B., Land Tenure and Social Organization: An Ethnohistorical Study from the Bolivian Oriente. *Inter-American Economic Affairs* (1960) 13, No. 4:44-66.
- HENSEL, WITOLD, and ALEKSANDRA BRONIEWSKA, *Starodawna Kruszwica*. 170 pp. Warsaw: Press of the Polish Academy of Sciences, 1961.
- HOBELL, E. ADAMSON, Three studies in African law. *Stanford Law Review* (1961) 13, No. 2:418-42.
- HOLAS, B., *Cultures materielles de la Côte d'Ivoire*. 96 pp., 60 plates. Paris: Presses Universitaires de France.
- HORKHEIMER, HANS, *Nahrung und Nahrungsgewinnung im vorspanischen Peru*. 155 pp. Biblioteca Ibero-Americana de Berlin. Berlin: Colloquium Verlag.
- IBARRA GRASSO, DICK EDGAR, Sobre el desarrollo de las civilizaciones precolombinas de Bolivia y el origen del Imperio Incaico. *Revista do Livro*. Rio de Janeiro: Instituto Nacional do Livro.
- IKEDA, JIRO, Contributions to the anthropology of Iraq. *Acta Medica et Biologica* (1960) 8, No. 2:109-29.
- IMRE, FERENCZI, Rákóczi alakja az abaúj-zepléni néphagyományban. *Ethnographia* (1960) 61:1-50.
- IZUI, H., Swadeshism in Japan. *Mathematical Linguistics* (1961) 16:2-9.
- JACOBETT, WOLFGANG, Der Stab des heiligen Wendelin. *Deutsches Jahrbuch für Volkskunde* 6:182-86. Berlin: Akademie-Verlag, 1960.
- KOSKINEN, AARNE A., *Aviki the first-born: An Analysis of a Polynesian chieftain title*. 191 pp. Helsinki: Academia Scientiarum Fennica, 1960.
- KOZLOWSKI, J. K., *Essai de classification des industries à pointes foliacées du Paléolithique supérieur Européen*. 131 pp. Krakow 1961. (In Polish, with French summary.)
- LASKER, GABRIEL WARD, *The evolution of man: a brief introduction to physical anthropology*. 239 pp. New York: Holt, Rinehart & Winston, 1961.
- LESER, PAUL, Felder und Bodenbaugeräte der Nyakvusa. *Ethnologica*, (1960) n.s., 2: 364-83.
- LESER, PAUL, *Plow complex, culture change and cultural stability*. Selected Papers of the Fifth Int'l. Congress of Anthropological and Ethnological Sciences (1956) (Ed. Anthony F. C. Wallace). Philadelphia: University of Pennsylvania Press, 1960.
- LITTLE, KENNETH, Applied anthropology and social change in the teaching of anthropology. *The British Journal of Sociology* 11, No. 4:332-48.
- LITTLE, KENNETH, Research Report No. 2 of the Department of Social Anthropology, The University of Edinburgh. *The Sociological Review* 8, No. 2:255-66.
- MAKARIUS, ROUL and LAURA, *L'origine de l'exogamie et du totémisme*. 382 pp. Paris: Librairie Gallimard, 1961.
- MANESSY, G., *La morphologie du nom en Bwamu*. 313 pp. Publications de la Section de Langues et Littératures, Université de Dakar, No. 4, 1960.
- MANESSY, G., *Tâches quotidiennes et travaux saisonniers en pays bwa*. 327 pp. Publications de la Section de Langues et Littératures, Université de Dakar, No. 5, 1960.
- MANGIN, WILLIAM, Drinking among Andean Indians. *Quarterly Journal of Studies on Alcohol* 18, No. 1:55-6.
- MANGIN, WILLIAM, Mental health and migration to cities: A Peruvian case. *Annals of the New York Academy of Sciences* 84: 911-17.
- MARTÍ, SAMUEL, *Canto, danza y música precortesianos*. 379 pp., 180 illustrations. Mexico, D.F.: Fondo de Cultura Económica, 1961.
- MEDELLIN ZENIL, ALFONSO, *Ceramics del Totonacapan: Exploraciones arqueológicas en el centro de Veracruz*. 217 pp., 111 illustrations. Xalapa: Universidad Veracruzana, 1960.
- MIKAMI, TSUGIO, *The dolmen and stonecist in Manchuria and Korea*. 18 pp., 4 plates. (Eng. trans.) Tokyo: Yoshikawa Kobunkan, 1961.
- MINIZ, SIDNEY W., and DOUGLAS HALL, *The origins of the Jamaican internal marketing system*. 26 pp. Yale University Publications in Anthropology, No. 57, 1960.
- MOBERG, CARL-AXEL, *Summary: Trends in the present development of quantitative methods in archaeology*. 33 pp. Göteborgs Universitets Årsskrift 67, 1961.
- MOORE, WILBERT E. and ARNOLD S. FELDMAN (Eds.), *Labor commitment and social change in developing areas*. 378 pp. New York: Social Science Research Council, 1960.
- MORSE, DAN, Prehistoric tuberculosis in America. *American Review of Respiratory Diseases* (1961) 83, No. 4:489-504.
- MUHLMANN, W. E., Situation actuelle de l'ethnographie et de l'ethnologie en Allemagne Occidentale. *Cahiers de l'Institut de Science Economique Appliquée* (1960) 103:179-225.
- NACHTIGALL, HORST, Die reliefkunst der San Agustín-Kultur (Kolumbien). *Baessler-Archiv*, n.s., 8:319-33.
- NAROLL, RAOUL, *Controlling data quality*. 6 pp. Symposia Studies Series No. 4 of the National Institute of Social and Behavioral Science. Washington: 1960.
- NAROLL, RAOUL, Two solutions to Galton's problem. *Philosophy of Science* (1961) 28, No. 1:15-39.
- NARR, KARL J., Sentido del arte Paleolítico. *Revista Iberoamericana Internacional* 4, No. 3:197-210.
- NATH, Y. V. S., *Bhils of Ratanmal*. 229 pp. Maharaja Savajirao University of Baroda Sociological Monograph Series 1, 1960.
- NOBACK, CHARLES R., *The heritage of the human brain*. 30 pp. New York: Amer. Mus. of Nat. Hist., 1959.
- Nouvelles feuilles archéologiques en Tchécoslovaquie*. 34 pp., 8 plates. Prague: Institut d'archéologie, Académie tchécoslovaque des Sciences, 1960.
- ORR, PHIL C., *Radiocarbon dates from Santa Rosa Island, II*. Bulletin No. 3 of the Santa Barbara Museum of Natural History. Santa Barbara: 1960.
- PAUVERT, J.-CL., L'éducation et le développement accéléré. *Civilisations* (1960) 10, No. 4:457-72.
- PAUVERT, J.-CL., *Migrations et éducation*. *Bulletin de l'I.F.A.N.* (1960) 22, Ser. B, Nos. 3-4:467-75.
- PAUVERT, J.-CL., Quelques aspects de la formation des leaders locaux. *International Review of Community Development* (1959) 1, No. 3:15-24.
- PRUFER, OLAF H., "Early Man East of the Mississippi," in *Festschrift für Lothar Zoltz*. pp. 421-55. Bonn: 1960.
- PRUFER, OLAF H., The Abri Schmidt, an important Upper Palaeolithic site in Bavaria. *Ohio Journal of Science* 61 (1961), No. 1:45-59.
- RAPOSO FONTENELLE, L. F., *A dinâmica dos grupos domésticos do Arraial do Cabo*. 42 pp. Rio de Janeiro: Serviço Social Rural, 1960.
- RAPOSO FONTENELLE, L. F., *Rendas e rendeiros do Arraial do Cabo*. 11 pp. Rio de Janeiro, Brazil: Serviço Social Rural.
- REINA, RUBEN E., Chinaulta, a Guatemalan Indian community. *Middle American Research Institute Publication* 24:55-130. New Orleans: Tulane University.
- RIAD, MOHAMMED, The Jukun: An example of African migrations in the sixteenth century. *Wiener völkerkundliche Mitteilungen* 7, Bd. 2, Nos. 1-4: 37-44.
- RIAD, MOHAMMED, Native plough in Egypt. *Bull. de la Société de Géographie d'Égypte* (1960) 33:241-77.
- SCHOBINGER, JUAN, A cien años del nacimiento de la antropología prehistórica. *Revista de la Universidad de Buenos Aires* (1960) 5, No. 1:6-113.
- SCHOBINGER, JUAN, *El "Oreopithecus" y el problema del origen del hombre*. 36 pp. Sociedad Amigos de la Arqueología Pub. No. 5. Mendoza: 1960.

SCHOBINGER, JUAN, Esquema de la prehistoria argentina. *Ampurias* (1958) 21:29-67.

SCHOTT, RÜDIGER, Der Entwicklungsgedanke in der modernen Ethnologie. *Saeculum* 12, Heft 1, 1961.

SCHWARZE, DAVID MARTIN, *Geology of the Lava Hot Springs Area, Idaho*. 51 pp. Occasional papers of the Idaho State College Museum, No. 4, 1960.

SHANDS, HARLEY C., *Thinking and psychotherapy*. 319 pp. Cambridge: Harvard University Press, 1960.

SINGER, MILTON, "The expansion of society and its cultural implications," in *City Inevitable, A Symposium on Urbanization and Cultural Development in the Ancient Near East*. University of Chicago: 1960.

SMALLEY, WILLIAM A., *Manual of articulatory phonetics, Part One*. 217 pp. Tarrytown, N.Y.: Committee on Missionary Personnel, National Council of Christian Churches, 1961.

Studi e ricerche del Centro Nazionale Studi di Musica Popolare dal 1948 al 1960. 291 pp. Rome: Accademia Nazionale S. Cecilia, 1960.

STURTEVANT, WILLIAM C., *Anthropology as a career*. 20 pp. Washington: Smithsonian Publication No. 4343, 1960.

SWADESH, MAURICE, *Tras la huella linguística de la prehistoria*. Suplementos del Seminario de Problemas Científicos y Filosóficos, Second Series, No. 26. Mexico D.F.: Universidad Nacional de Mexico, 1960.

SWARTZ, MARC J., Negative ethnocentrism. *Journal of Conflict Resolution* 5, No. 1: 75-81.

SYMMONS-SYMONOLEWICZ, KONSTANTIN, Bronislaw Malinowski: Individuality as a theorist. *Polish Review* (1960) 5, No. 1:2-13.

SYMMONS-SYMONOLEWICZ, KONSTANTIN, The origin of Malinowski's theory of magic. *Polish Review* (1960) 5, No. 4:1-9.

SZABADALVI, JOZSEF, Die Ornamentik der Ungarischen Schwarzkeramik. *Acta Ethnographica* (1960) 9, Nos. 3-4: 1-79.

TEXTOR, ROBERT B., *From peasant to pedicab driver*. 83 pp. Yale University Southeast Asia Studies, Cultural Report Series No. 9, 1961.

THEODORSON, GEORGE A. (Ed.), *Studies in Human Ecology*. 626 pp. Evanston, Ill.: Rowe, Peterson & Co.

TOKAREV, S. A., Problem and method in kinship terminology. *Etnografii Polskiej* 3:366-71.

TOVAR, ANTONIO, *Catálogo de las lenguas de América del Sur*. 405 pp. Buenos Aires: Editorial Sud-americana, 1961.

TROGER, ERNEST, *Ban-Pae-Lungar*. Mitteilungen der Österreichischen Geographischen Gesellschaft 102, Heft 2:165-198.

VALOCH, KAREL, *Magdalénien na Moravě*. 105 pp. Brno: Krajské Nakladatelství v Brně, 1960.

VIDYARTHI, L. P. (Ed.), *Indian anthropology in action*. 144 pp. Council of Social and Cultural Research, Ranchi, Bihar, India, 1960.

VIDYARTHI, L. P., *The Sacred Complex in Hindu Gaya*. 232 pp. New York: Asia Publishing House, 1961.

WALLACE, ANTHONY F. C., Cultural composition of the Handsome Lake religion. *Bureau of American Ethnology Bulletin* (1961) 180:139-57.

WALLACE, ANTHONY F. C., On being just complicated enough. *Proceedings of the National Academy of Sciences* (1961) 47, No. 4:458-64.

WALLACE, ANTHONY F. C., "The psychic unity of human groups," in *Studying Personality Cross-culturally*, pp. 129-63. Evanston: Row, Peterson & Co., 1961.

WALLRAF-RICHARTZ-MUSEUM OF KÖLN, *Kunst und Kultur der Hethiter*. 228 plates with text. Köln: 1960.

WALTER, HEINZ, Eine wenig bekannte Fischfangmethode in Südost-Bolivien. *Baessler-Archiv*, New Series, 8:79-82. Berlin 1960.

WETHERINGTON, RONALD K., An alternate method of assessing skeletal maturity from the hand and wrist. *Papers of the Michigan Acad. of Sciences, Art and Letters* (1961) 46:419-33.

WHETTEN, NATHAN L., *Guatemala: the land and the people*. 232 pp. New Haven: Yale University Press, 1961.

WILBERT, JOHANNES, Nachrichten über die Curipaco. *Ethnologica* (1960) New Ser., 2:508-21.

WORSLEY, PETER, *La tromba suonera: I culti millenaristici della Melanesia*. 362 pp. Turin: Tipografia Toso, 1961.

WURM, S. A., *The linguistic situation in the Highlands Districts of Papua and New Guinea*. 23 pp. Canberra: C'wealth Gov't. Printer, 1961.

WURM, S. A., The question of aboriginal place names. *Cartography* (1960) 3, No. 3:134-139.

Wanted

► For scientific investigation of the South American arrow poison, curare, the Museum für Völkerkunde in Vienna requires a few grams of original tubocurare, as the Indians have prepared it for 50 to 100 years, with exact designations of the tribes concerned and unambiguous localization of the areas where the specimens were acquired and the dates of acquisition. The specimens should be sent to the address given below.

The Museum also requests all public collections, museums, and private collectors who possess calabash-, pot-, or tubocurare to submit the following information, as far as it can be obtained from inventories, documents, etc.:

- (1) Place of acquisition of the particular curare arrow-poison (exact geographical location).
- (2) Name of the tribe from which the poison was procured.
- (3) Date of acquisition.
- (4) Person who acquired it.
- (5) Whether the tribe from which the poison was procured prepared the curare; if not, from whom it was traded or purchased.
- (6) Any other information about the preparation of the poison, as far as it is known.

Replies and specimens should be sent to:—ING. WILHELM P. BAUER, Museum Für Völkerkunde, Neue Hofburg, Wien I, Austria.

► Information on legislation existing in various countries to ensure the preservation of important ancient monuments, including details of official and voluntary bodies which record archaeological sites, and the procedures followed to safeguard them from vandal-

ism.—P. GATHERCOLE, Hon. Secretary, New Zealand Archaeological Association, Otago Museum, Dunedin, New Zealand.

► Earlier publications of Associates in CURRENT ANTHROPOLOGY. I should welcome brief summaries of these, or lists of publications.—ISTVÁN KACSUR, Kosuth Lajos Tudományegyetem, Ember-tani Intézet, Debrecen, 10, Hungary.

► Contact with other research workers interested in kinship in French Canadian communities or in the more general study of kinship among North American communities of European origin, particularly those of French descent in the southern U.S.A. In 1962 I propose to undertake further research on kinship as an integrating factor in French Canadian communities, particularly those of Western Canada.—RALPH PIDDINGTON, University of Auckland, Auckland, New Zealand.

► Contact with anthropologists who have just finished field work in Chiapas, México, and with those who are now doing research in this area or plan to do so in the next year. I have returned to Chiapas to continue research on Highland Maya political organization, and am particularly interested in studies bearing upon tribal organization (contemporary as well as pre-Conquest).—HENNING SIVERTS, Instituto Nacional Indigenista, Centro Coordinador Tzeltal-Tzotzil, San Cristóbal Las Casas, Chiapas, México.

Institutions

THE CENTER OF ETHNOLOGICAL STUDIES (Centro de Estudos Etnológicos) at São Paulo, Brazil, was established to bring together students at the University of São Paulo and other persons who are engaged in ethnological research or whose main interests are ethnological. Its immediate goals include a detailed ethnographic survey of Brazilian Indian tribes; studies of the development of tribal cultures; the promotion and assistance of field studies and investigations; and the proposal of suitable measures for preserving native cultures or for guiding them through the transition to their integration into Brazilian culture.

To implement these objectives, the Center has instituted a program that includes the following measures: (1) detailed studies of tribal groups by research teams composed of ethnologists, sociologists, anthropologists, linguists, doctors, and other specialists; (2) assembling archives of ergological, photographic, and sound material, to be de-

posited in the offices of the C.E.E. where they will be available to interested persons; (3) publication of its research; (4) translation and publication of books and journal articles of particular interest; (5) participation in meetings and discussions that fall within the scope of its objectives; and (6) exchanges with other local associations inside or outside Brazil. Correspondence with the Center may be addressed to: Center of Ethnological Studies, Faculdade de Filosofia, Universidade de São Paulo, Caixa Postal 8105, São Paulo, Brasil.

Conferences

The Teaching of Anthropology

August 9-16, 1960

at Burg Warthenstein, Austria

Sponsored by The Wenner-Gren Foundation for Anthropological Research

Organizing Chairman: D. G. Mandelbaum, University of California, Berkeley, U.S.A.

Co-Chairman: K. L. Little, University of Edinburgh, Scotland

Rapporteurs: Ethel M. Albert, University of California, Berkeley; and Gabriel W. Lasker, Wayne State University, Detroit, Mich., U.S.A.

Participants, papers, and discussants:

D. G. MANDELBAUM, "Report on educational resources in anthropology project"; Discussant: DARYLL FORDE, University College, London.

MARGARET MEAD, American Museum of Natural History, New York (not in attendance). "The objectives of the teaching of anthropology as part of a liberal education"; Discussant: S. C. DUBE, University of Saugar, India.

RAYMOND FIRTH, London School of Economics and Political Science, "Aims and methods in the teaching of social anthropology"; Discussant: VINIGI L. GROTTANELLI, Istituto per le Civiltà Primitive, Università di Roma.

GUTORM GJESSING, Universitets Etnografisk Museum, Oslo, Norway, "Aims and methods in the teaching of archaeology and culture history"; Discussant: R. F. HEIZER, University of California, Berkeley.

F. G. LOUNSBURY, Yale University, New Haven, Conn., U.S.A., "Aims and methods in the teaching of linguistics"; Discussant: R. L. BIRDWHISTELL, Eastern Pennsylvania Psychiatric Institute, Philadelphia, Penna., U.S.A.

W. S. LAUGHLIN, University of Wisconsin, Madison, Wisc., U.S.A., "Aims and methods in the teaching of physical anthropology"; Discussant: G. W. LASKER.

R. L. BIRDWHISTELL, "Techniques and technical aids in the teaching of anthropology"; Discussant: D. G. MANDELBAUM.

MEYER FORTES, University of Cambridge, "Main principles in training professional anthropologists and in directing graduate student research"; Discussant: MONICA H. WILSON, University of Cape Town, South Africa.

J. H. M. BEATTIE, Oxford University (not in attendance), "Techniques of graduate training: seminars, examinations"; Discussant, F. G. LOUNSBURY.

K. L. LITTLE, "The place of applied anthropology in undergraduate and graduate teaching"; Discussant, JACQUES J. MAQUET, Université Officielle du Congo et du Ruanda-Urundi, Elisabethville, Katanga, Congo.

Discussion:

Building on the papers and discussions presented at 8 previous meetings held in the U.S.A. as part of the Educational Resources in Anthropology project,¹ this symposium met to explore topics that had not been sufficiently covered in the earlier sessions, and to provide opportunity for comparison of the teaching of anthropology in several countries.

1.—*General comments.* The different traditions of anthropology and institutional settings for teaching represented at the conference caused some difficulties in communication and comparison. Nevertheless, certain common ideas about teaching, as well as certain sharp differences, were evident, particularly regarding the scope of the subject and the kinds of students toward whom teaching should be primarily directed.

2.—*Culture change and anthropological teaching.* Given the rapid changes in the modern world, can anthropologists continue to place major emphasis on isolated "primitive" groups? Most participants agreed that a purview more inclusive than the former focus on non-urbanized, non-literate societies was not only desirable, but was necessary for anthropological teaching. It was also noted that the study of anthropology is often a means of self-discovery for students, because of the objective view taken of one's own culture and the objective comparisons made among cultures.

One consequence of the spread of literacy and of anthropological research among literate people is that anthropological works are increasingly being read by persons belonging to the societies which are examined in them. Such readers, Dube pointed out, find many of these works unsatisfactory because they lack the perspective of the "insider." He suggested that, for Asian and African students, such research and publications would be more effective if produced by co-operation between members of the culture being studied

¹ A subsequent "General Conference on the Teaching of Anthropology" was held in Berkeley, California, March 2-4, 1961, in which North Americans who had reviewed the results of all the previous meetings developed and discussed new papers on the subject. The results of these ten conferences on teaching will be published.

and anthropologists from other cultures.

Students of anthropology in Asian and African countries require special adaptations of anthropological teaching. Wilson and Maquet noted that the subject matter usually considered cultural and social anthropology by Westerners is viewed in Africa as history and sociology; the same is true of India. Teaching in all countries, it was agreed, must be flexible and attuned to the needs of students, rather than fixed by some arbitrary curriculum standard.

3.—*Specialization and external relations.* Some participants maintained that the place of anthropology in higher education must be central, because anthropology is the only science that looks on man and culture as a whole. Others insisted that so broad a conception was not conducive to the effective teaching of a scientific discipline.

A recurrent objection to the broader conception of anthropology came from those who argued that specialization has not proceeded so far as to require, for effective teaching, the separation of fields which were formerly grouped together. This argument was countered by those who believe that teaching anthropology on a broad intellectual front repays in stimulation and transfer of concepts what may be lost by the wider spread of teaching effort; each branch of anthropology gains from being taught as one integral part of the general subject. Heizer showed how this was true in archeology; similar evidence was adduced for linguistics and physical anthropology.

Some differences about teaching policy seemed to arise from different national contexts of teaching. Thus, in the United States many thousands of undergraduates take courses in anthropology, but generally devote a relatively short time to it, viewing it as part of their general education. In England, there are only several hundred students of anthropology, but most of them concentrate on the subject. Hence, one English anthropologist saw the main concern of anthropological teaching as "bringing up our successors" in the profession, while an American saw it as bringing up a generation of educated citizenry.

4.—*Applied anthropology.* Discussion of this field evoked the somewhat metaphysical question of its existence as a legitimate teaching subject. A number of activities called applied anthropology appear to lack the kind of coherent unity in concept or method which might justify a special training program. Maquet pointed out that in applied programs it was usually necessary to draw from many different sources in addition to anthropology. Little took the position that good training in social

anthropology is the best training for applied work in social problems. This field of inquiry is undoubtedly lively and growing, but there is not yet general agreement about its proper place in teaching.

5.—*Teaching aids.* Birdwhistell's central idea, that such audiovisual aids as films, slides, and recordings are truly aids to teaching only if they are properly used, was unanimously endorsed. Properly used, they do not necessarily save the teacher's time—students must be prepared by explanatory lectures—but can enhance the effectiveness of teaching. Educational films require extensive planning and co-operation with various technical specialists, yet the few good films now available are so useful in teaching as to arouse considerable interest in making others of similar quality.

6.—*Teaching objectives and curricula.* Fortes noted that the graduate student should choose one area to master. In graduate training scholarship and

knowledge of the relevant literature should be emphasized; in preparation for field research, graduate students should acquire a "built-in system of expectation" rather than any particular set of field-work techniques.

A main problem in the teaching of undergraduates is whether the first course should be a general survey of anthropology in which many topics and problems are treated briefly, as Fortes recommended, or whether beginning students should be given a narrower but deeper introduction to one phase of anthropological study. Lounsbury held that breadth should come after depth and that the relevance of other fields to one's speciality is best grasped by the student after he has done intensive work. Most participants, however, favored a survey approach to an introductory course in anthropology, if only to acquaint the student with the general nature of the subject.

7.—*Extra-scientific involvement.* Gjesing's paper, illustrating the influence

of nationalism on archeology, demonstrated that, even in this field, teaching could involve political and ethical factors. Teaching about race draws the physical anthropologists into areas of political interest. Laughlin attested that the most effective teaching about race presents the scientific facts and analysis and does not exhort. Lasker noted the need to include race relations in the studies of physical anthropologists, who may concentrate so much on biological aspects that they do not realize the social implications of their subject.

The moral and ethical components of teaching were briefly examined. The importance of the study of anthropology in imparting respect and tolerance for other cultures was recommended by several discussants. But others questioned whether students who chose to study anthropology were not already predisposed to a respectful view of all mankind. All agreed that the study of anthropology could and should affect the total outlook of the student.

Journal Contents

(Continued from p. 504)

Man

Royal Anthropological Institute, 21 Bedford Square, London, WC1, England.

Vol. 61, Articles 101-130

DRS. H. LEHMANN and J. G. ROSS: Haemoglobin Phenotypes in Nigerian Cattle.

CORNFIELD JEST: A Technical Note on the Tibetan Method of Block-Carving.

DR. GROVER S. KRANTZ: Pithecanthropine Brain Size and its Cultural Consequences.

B. M. FAGAN: A Note on Potmaking among the Lungu of Northern Rhodesia.

K. R. ROBINSON: A Note on Hollow-based Pottery from Southern Rhodesia.

Volume 61, Articles 146-164, July, 1961

DR. R. BROTHWELL: An Upper Palaeolithic Skull from Whaley Rock Shelter No. 2, Derbyshire.

PROFESSOR E. E. EVANS-PRITCHARD: Zande Clans and Totems.

H. R. H. PRINCE PETER OF GREECE AND DENMARK, DR. KNUD ELDON and DR. J. B. JØRGENSEN: The ABO and Rhesus D Groups of 304 Tibetans, Tested on Eldoncards.

Man in India

Manager, Man in India, 18 Church Road, Ranchi, Bihar, India.

Volume 41, No. 1, January-March 1961

NIRMAL KUMAR BOSE: Crime: Its Origins. K. N. BHARGAVA and G. MALAVIYA: Variations in the First Metatarsal Bone.

BHUBAN MOHAN DAS and PRIYABULA UZIR: A Study of the Khasi foot.

BISWANATH BANDOPADHYAY: Hira Potters of Assam.

PABITRA GUPTA and PRATAP CHANDRA DUTTA: Lower Palaeolithic Industry of Amirdhamangalam, Chinglelaput, Madras.

G. C. MOHAPATRA: Jaspert as Raw Material for Stone Age Artifacts in Eastern India.

DORA SCARLETT: Mudichur.

Mankind

Anthropological Society of New South Wales, c/o City of Sydney Library, Sydney, N.S.W., Australia.

Volume 5, No. 10, June 1961

A. MASSOLA: A Victorian Skull-cap Drinking Bowl.

F. D. MCCARTHY: The Story of the Mungan or Bagadjimbiri Brothers.

J. H. BELL: Some Demographic and Cultural Characteristics of the La Perouse Aborigines.

Oceania

Editor: A. P. Elkin, University of Sydney, Sydney, N.S.W., Australia.

Volume 31, No. 3, March 1961

A. MASSOLA: In Memoriam Leonhard Adam.

A. P. ELKIN: The Yabuduruwa.

H. S. KITCHING: Observations of Customs Associated with Kadijja Practices in Central Australia.

A. A. ABBIE: A Preliminary Survey of the Growth Pattern of Central Australian Aboriginal Males.

A. E. P. GRIMES and SHUN-KEUNG LEE: A Survey of Blood Groups in Hong Kong Chinese of Cantonese Origin.

Sovetskaya Etnografiya

Institute of Ethnography, Academy of Sciences of the U.S.S.R., 1^o Cherenushkinkaia, No. 19, Moscow, U.S.S.R.

No. 1, 1961

M. G. LÉVINE: L'ethnologie et l'anthropologie comme source des études historiques.

A. I. ROBAKIDZE: Nouveaux traits des habitations paysannes d'aujourd'hui en Géorgie.

B. A. KALOËV: De l'histoire des liens économiques et culturels russo-tchéchènes.

S. M. ABRAMSON: Évolution économique et culturelle des Kasakhs pendant les années de l'édification socialiste.

S. A. AROUTHOUNOV and A. I. MOUKHLINOV: Matériaux pour la classification ethno-

linguistique des peuples de Vietnam.

V. A. KATZMAN: Évolution de la différenciation économique des paysans africains de Tanganyika après la seconde guerre mondiale.

E. A. TARVERDOVA: Particularités spécifiques de l'Islam en Soudan occidental aux XII-XVI ss.

J. A. FEDEROV: Sur la question des premiers états d'ethnogénèse des peuples de Daguestan.

K. G. GOUSLISTY, V. F. GORLENSKO: De la périodisation de l'histoire de l'ethnographie ukrainienne.

No. 2, 1961

G. F. DEBETZ: Certains aspects des transformations somatiques de l'Homme sapiens.

L. N. TÉRÉNTIÉVA: Formation de nouvelles traditions dans le mode de vie des kolkhoziens-lettons.

Y. S. SMIRNOVA: Coutume "d'éviter la rencontre" chez les Adygués et sa disparition à l'époque soviétique.

I. A. JNANKO: Problème de la population mi-nomade dans l'histoire de l'Asie Centrale et du Kazakhstan.

M. G. LÉVINE: Quelques problèmes d'anthropologie ethnique du Japon.

P. N. TRÉTIKOV: Aux sources de l'histoire ethnique des peuples magyaro-finnois.

E. V. POMÉRANTZEVA: Premières publications des contes satiriques populaires russes.

Word

Linguistic Circle of New York, Columbia University, New York 27, N.Y., U.S.A.

Volume 17, No. 1

ARCHIBALD A. HILL: Grammaticality.

JOHN D. MILLER: Word Tone Recognition in Vietnamese Whispered Speech.

WILLIAM J. SAMARIN: The Vocabulary of Sango.

F. R. PALMER: Relative Clauses in Tigre.

PAUL GARDE: Réflexions sur les différences phonétiques entre les langues slaves.

Research Reports

The Kisoro Pattern of Mountain Gorilla Preservation

Late in 1957, the University of the Witwatersrand Research Committee began giving financial support to the research upon the mountain gorilla in western Uganda initiated by Mr. Walter Baumgartel. That support has enabled him to maintain, during the intervening 4 years, an African guide and two trackers, by means of whom virtually continuous contact is maintained with the gorillas on the northern slopes of the Birunga volcanoes and they can be shown to visitors.

Despite its present fame (*vide* Zahl 1961), and the many spectacular scientific results that have emerged in such rapidity from this enterprise, it is easy—unless some of the facts are put on record in perspective—to lose sight of its total importance. The success with which the Kisoro Gorilla Sanctuary has been hitherto maintained has demonstrated the desirability of retaining such guides and trackers as long as possible; however, this arrangement may prove too delicate to survive this troubled period of social transition in Central Africa. Meanwhile, the success of the Kisoro arrangement has also shown the value of actively promoting the establishment of similar or improved arrangements for the preservation of the African great apes in other areas.

Previous articles (Dart 1958, 1959) have described the origin of the Research Committee's project and its immediate incidental facilitation of higher primate field studies that were already projected by Japanese (Imanishi 1958, Baumgartel 1958) and American scientists (*vide* Opportunities and needs for field-research on the biology of primates in tropical Africa: summary of meeting 4-5 December 1959 in the National Academy of Sciences, Washington, D.C., sponsored by The New York Zoological Society). With the aid of a grant from the Gorilla Research Unit that had been set up in Johannesburg in 1957, the *South African Journal of Science* was able to publish Miss J. H. Donisthorpe's (1958) pilot study of the Kisoro Gorilla Sanctuary situation, area, vegetation, animals, and weather; as well as of the gorillas, and her method of tracing the size and movements of groups by their evening habit of making new nests.

This initial long-term study within a restricted area of the mountain gorilla's natural habitat reoriented the attitude of scientists toward the possibilities of carrying out such gorilla habitat studies successfully.

Shortly after his preliminary studies on baboon and lemur behaviour, conducted in the Transvaal and Madagascar, Dr. Niels Bolwig began work on the gorilla and chimpanzee in Uganda and the Congo. This enabled him to assist Professor John T. Emlen and his graduate student George B. Schaller at Kisoro in March 1959. After a 6-months investigation of the distribution of mountain gorillas in Uganda and Congo, Professor Emlen returned to the U.S.A. Mr. Schaller, in the meantime, had discovered a more suitable terrain for his behaviour studies in the vicinity of Mount Mikeno, where he stayed until the middle of 1960, when disturbances following independence interrupted his long-range field studies and led him to spend the remaining months of his two-year programme doing comparative research on orang-utan in Indonesia.

Dr. Bolwig, however, had become so impressed with Uganda as an area for the study of primates in general, and with the Kayonze forest area somewhat north of Kisoro for studies of the gorilla and chimpanzee, that he joined the Zoology Department of Makerere University College. Since then he has built a hut for field work in the Kayonze forest with the assistance of a grant from the Uganda Game Department, which has assisted in the establishment of further hut accommodation in the Kisoro game sanctuary also.

The emergence of this local pride and greater personal interest, as well as the research upon gorillas in Uganda itself and the facilitation of behaviouristic studies by Japanese and American scientists, would have been more than satisfactory rewards for the assistance given to the Kisoro Sanctuary by the Research Committee of the University of the Witwatersrand. In fact, the work of Emlen and Schaller in the Congo had been so successful that it almost appeared to some that further work at Kisoro might be unnecessary. But the sudden interruption of Schaller's work and the parlous immediate future of the Congo and its crucial fauna has shown everyone the importance of retaining the Kisoro arrangement. This had also become manifest to members of the research unit through a succession of further discoveries at Kisoro, concerning not only the habits of gorillas but also their behaviour when confronted with the death of a member of their band.

The first of these deaths, in August 1958, was that of the venerable old male known by the guide Reuben and his trackers as the "Saza Chief." It was the outcome of a 12-day struggle between two leaders of separate gorilla groups. Then, in the early part of 1960, the male

who had been responsible for the death of the "Saza Chief" acted strangely by leaving his family of five females and one small male gorilla. Taking the infant male with him, he threatened anyone who approached them. The females were usually found feeding some distance away, and the old male showed signs of increasing ill health. On February 22 a party of visitors led by Reuben found the male dead in the flattened undergrowth with his young companion crouched alongside, abandoned by the five females. The party succeeded in capturing the infant despite his great strength, and took him down the mountain. Later, he was taken to Entebbe and is today in the Regent's Park Zoological Garden, London.

Seven months later, in September 1960, Reuben found the bones of a third gorilla, a young female, still partially covered with flesh. These proved to comprise an almost complete skeleton, with the skull still well-preserved. This specimen was handed over by the Game Department to Makerere Medical College, where it is available, along with the skeleton of the "Saza Chief," for comparative skeletal studies.

Despite regular visits and searches, encounters with the remaining gorillas became less successful than previously. The family that had consisted of the five females seemed especially nervous. A male that had attached himself to the band was timid; but one of the females (who usually are the more timid) attacked Reuben and attempted to strangle him. These alterations from their previous behaviour were most puzzling.

About six months later, on February 14, 1961, attracted by unusual excitement among a number of birds, Reuben and his men found the body of another male gorilla, apparently killed by a leopard only a short while before. They followed a trail uphill over the flattened vegetation for 50 yards to the place where the gorilla group had spent the previous night. Judging from the nests, it had been the same family. The leopard had apparently sprung onto the male while he was still asleep. The fierce ensuing fight sent them rolling down the slope locked together and ended in the death of the gorilla, who had severe wounds in his neck and a huge gash in his right groin and belly. The following day, Mr. Craggs arrived from Makerere College in time to inject the corpse and take it to Kampala virtually intact for dissection at the Medical College.

Three days after finding the body of this male gorilla, Reuben and his assistants disturbed a huge leopard eating the putrid flesh of another gorilla, apparently a female, which must have

been killed shortly before or after it had left its other victim. As little doubt now remained of its danger to the 40 or 50 gorillas estimated to inhabit the Kisoro Sanctuary, permission was given to destroy the leopard. On March 16, 1961 it was still at large despite attempts to trap it and forays into its territory by two European hunters with the trackers. The last expedition, however, brought word that twice they had seen four females with an infant, a baby, and a new silver-backed male, apparently a fresh arrival from over the mountain in Ruanda-Urundi.

At first it was thought that the sequence of events leading to the present family of seven was that the five widows of the deceased male had attached themselves to another family—a male, female, and infant—to form a family of nine, i.e., 7 adults, an infant, and a new baby. But it now appears that the females moved away and had been replaced by this new group from the Congo side of the mountain.

In addition to the light they have thrown on gorilla life, these occurrences have resulted in the unexpected salvation for scientific study of a live infant gorilla, a properly preserved adult male body, a number of pelages, and at least 3 complete male and 2 fairly complete female skeletons of the mountain go-

rilla. This behavioural knowledge, as well as the specimens, would have been lost had it not been for the financial support that enabled Baumgartel's unique observational system to remain in continuous existence over the past 4 years.

Further, the support from the Research Committee of the University of the Witwatersrand has demonstrated that, apart from mankind, the most destructive enemy of the great apes as of other primates in Africa is the leopard. It has incidentally directed the attention of all those concerned with preserving the mountain gorillas in their natural habitat to the importance not only of retaining the existing arrangements, but of developing more systems like that Baumgartel has used at Kisoro, for keeping continuous human contact with gorillas and their movements. In that way not only can their behaviour be observed, but their well-being ensured and their bodies preserved for study when they die.

References

- BAUMGARTEL, W. M. 1958a. Playing host to gorillas. *Uganda Wild Life and Sport* 1:21-26.
- . 1958b. The Muhavura gorillas. *Journal of Primatology* 2:79-83.
- . 1959. The last British gorillas. *The Geographical Journal* 32:32-41.
- . 1960. *König in Gorillaland. Geschichten um mein Urwaldhotel*. Stuttgart: Frankh'sche Verlagshandlung.
- DART, R. A. 1958. The heart of Africa. *South African Journal of Science* 54:192-94.
- . 1959. Progress in investigating African apes in their natural habitat. *South African Journal of Science* 55:305-10.
- . 1960. Can the mountain gorillas be saved? *Current Anthropology* 1:330-33.
- DONISTHORPE, J. H. 1957. A pilot study of the mountain gorilla (*Gorilla gorilla Beringei*) on south-west Uganda, February to September 1957. *South African Journal of Science* 54:195-217.
- EMLEN, J. T. 1960. Current field studies of the mountain gorilla. *Current Anthropology* 1:332; *South African Journal of Science* 56:88-89.
- EMLEN, J. T., and G. B. SCHALLER. 1960. Distribution and status of the mountain gorilla (*Gorilla gorilla Beringei*) 1959. *Zool. Sci. Cont. New York Zoological Society* 45:41-52.
- GALLOWAY, A. et al. 1959. The study of *Gorilla gorilla Beringei* with a post-mortem report. *South African Journal of Science* 55:205-09.
- JARVIS, C. 1960. The Zoo's most valuable baby. *Zoo Magazine* (Regents Park, London), Summer no.
- ZAHL, P. A. 1960. Face to face with gorillas in central Africa. *National Geographic* 117:114-37.

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Calendar

1961

December 19-23. 7th Inter-American Congress of Psychology, Monterrey, Mexico. Write: Centro de Investigaciones Sociales, A. C., Box 7553, University of Texas, Austin, Texas, U.S.A.

December 27-30. Society for the History of Technology, annual meeting, Washington, D. C., U.S.A. Write: Paul Leser, 55 Elizabeth Street, Hartford 5, Connecticut, U.S.A.

December 28-30. American Association for the Advancement of Science. Section H (Anthropology) annual meeting. Denver, Colorado, U.S.A. Write: Program Chairman, D. M. Pendergast, Department of Anthropology, University of Utah, Salt Lake City, Utah, U.S.A.

1962

May 3-5. Society for American Archaeology, 27th annual meeting. Tucson, Arizona, U.S.A. Program Chairman: W. W. Wasley, Arizona State Museum, University of Arizona, Tucson, Arizona, U.S.A.

August 19-25. XXXV International Congress of Americanists. Mexico City, Mexico. Write: Dr. Miguel León-Portilla, General Secretary, Instituto Indige-

nista Interamericano, Niños Héroes No. 139, México 7, D. F.

August 29-September 3. 6th International Congress of Prehistoric and Protohistoric Sciences. Rome, Italy. Write: L. Cardini, Musee Preistorice Etnografice "L. Piguerini," Via del Collegio Romano 26, Rome, Italy.

September 2-8. 5th World Congress of Sociology. Washington, U.S.A. Write: International Sociological Association, Skepper House, 13 Endsleigh Street, London W.C. 1, England.

September. 9th International Congress of Linguists. Cambridge, Mass., U.S.A. Write: Miss Chr. Mohrmann, 40 Sint Annastraat, Nijmegen, Netherlands.

November 15-17. American Anthropological Association. 61st Annual. Chicago, Illinois, U.S.A.

1963

11th International Congress for the History of Religions. India. Write: C. J. Bleeker, 290 Churchill-lann, Amsterdam, Netherlands.

November 21-23. American Anthropological Association. 62nd Annual. San Francisco, California, U.S.A.

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Continué du couvert premier intérieur
ter l'article lui-même, des commentaires précédés du nom de leur auteur et une réponse à ces commentaires. Après publication de l'article, la revue accepterait les commentaires qui n'auront pas fait l'objet d'une demande particulière. Ils seront publiés pour compléter les commentaires prévus par l'auteur ou la rédaction, afin d'élargir le champ de la discussion.

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La section "nouvelle" contiendra des informations importantes d'intérêt général: avis et compte-rendu de réunions et de conférences, annonce des grandes découvertes, indications de bourses et de subventions. Par documentation de base, on entend les bibliographies des bibliographies imprimées, des index de catalogue de recherche, des répertoires de spécialistes et des institutions scientifiques dans différents disciplines an-

thropologiques, des listes de thèses de doctorat et de recherches en cours, ainsi que toute espèce d'information utile. On y ajoutera des demandes de renseignement nécessaires à des recherches précises, et des suggestions quant aux recherches à entreprendre.

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